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ON THE PROPORTIONAL CONTRIBUTIONS OF DIFFERENCES IN NATURE AND IN NURTURE TO DIFFERENCES IN INTELLIGENCE*

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I

Edward L. Thorndike (34) once wrote that in one sense nothing in human nature is due either to heredity or to environment.

> But in another sense the most fundamental question for human education asks precisely that we assign separate shares in the causation of human behavior to man's original nature on the one hand and his environment or nurture on the other. In this sense we neglect, or take for granted, the cooperating action of one of the two divisions in order to think more successfully and conveniently of the action of the other. . . . The custom of thus abstracting out the original nature of man in independence of any and all influences upon it is so general and so useful that it is best to follow it throughout.

Similar programmatic statements have been made from time to time subsequently. An ambitious restatement is that of Shuttleworth (30), who begins his discussion of the nature-nurture problem with the question, "What promises to be the most strategic method or methods of improving the health, intelligence, and general well-being of mankind over the next fifty years?" He feels that a related but more immediate question is, "What are the relative contributions of hereditary differences and of environmental differences in accounting for individual differences in respect to each significant human variable?" Shuttleworth plainly means that percentage values are to be assigned to the contributions of heredity and environment.

A considerable number of the most important investigations

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in the field of individual differences have been oriented towards the discovery of the numerical value of the contributions of differences in heredity and differences in environment to differences in intelligence for a specified population. The purposes of the present paper are to review briefly these studies and the major criticisms of them, to add a critique of an assumption common to all of the studies, and to evaluate the problem to which they are directed.

II

In his 1905 monograph on twins, Thorndike (33) claims to present

> the results of precise measurements of fifty pairs of twins from 9 to 15 years old in six mental traits and their bearing upon the comparative importance of heredity and environment as causes of human difference in intellectual achievement. They will be found to give well-nigh conclusive evidence that the mental likenesses found in the case of twins and the differences found in the case of non-fraternal pairs, when the individuals compared belong to the same age, locality and educational system, are due, to at least nine tenths of their amount, to original nature.

The combination of correlational methods and intuitive judgment which Thorndike used has not played an important part in subsequent approaches to the problem. And it is not surprising that the following thirty-five years, which saw so elaborate a development of mental measurement and statistical analysis, have changed our conception of "well-nigh conclusive evidence."

A second attempt to quantify the components of mental test scores was Burt's (6) famous, ill-fated regression equation. Burt assumed that mental age on the Binet-Simon scale (B) could be expressed as a linear combination of educational age (S), intellectual development in terms of years (I), and chronological age (A). From his data on the correlation of these factors he derived the following regression equation:

$$B = .54S + .33I + .11A$$

Burt's interpretation of the regression coefficients as representing proportional contributions of school experience, native endowment, and chronological age to mental test score was challenged by Holzinger and Freeman (15). They point out that he assumed (a) that his tests were pure tests of what they proposed to measure and (b) that what they measured was a direct cause of mental age. In order to show that Burt's interpretation was not legitimate without such assumptions, they computed the regression of chronologi-

cal age on the other variables, and showed that, by Burt's method, one would be led to conclude that half of chronological age was attributable to school attainment. Correlation is not a direct measure of causal relationship because the association may be due to unmeasured variables causally related to both the correlated variables.

Although he was concerned with physical rather than mental traits, an early paper by Fisher (**9**) is of interest because he introduced methods which have been used subsequently with some modifications. He utilized the correlations between relatives (parent-child, siblings, etc.) to "calculate the numerical influence . . . of the total genetic and non-genetic causes of variability."

In order to perform the calculations, he assumed cumulative (i.e., additive) Mendelian factors as causes of the traits studied, and he assumed that environment, whose effect is added to that of heredity, always works in a random fashion, thus always reducing the correlations. Of importance is Fisher's use of a ratio of variances to describe proportional contributions. The proportional contributions of a factor *A* to the variance of a trait *B* is the ratio of the variance of *B* when factors other than *A* are held constant to the total variance of *B*. Using obtained correlations for stature, Fisher found that the variance could be apportioned into 54 per cent due to the variance of ancestry, 46 per cent due to the variance of a sibship, or it could be apportioned into 83 per cent due to the variation in genotype and 17 per cent due to the association of factors by homogamy. Admitting the possibility of sampling errors in the original correlation coefficients, he concluded that "it is very unlikely that so much as 5 per cent of the total variance is due to causes not heritable."

Burks (**1**) has criticized the assumption that the effects of environment are random, especially in application to intelligence. Hogben (**12**) has insisted that the assumption applies neither to physical nor to other characteristics: "The viviparous habit of the human species, the existence of the family as a social institution, the stratification of human society in widely different social levels all conspire to create a framework of environment which is intimately related to the distribution of genetic differences."

Willoughby (**38**) gave 11 standard group intelligence and achievement tests to parents and children of 141 families living in the vicinity of Palo Alto. The average of the parent-child correlations, corrected for attenuation, was .35; the average of the corrected sibling correlations was .42; the average corrected husband-wife correlation was .44. Using the formula derived by Fisher, Willoughby computed the contribution of environment to these

mental test abilities to be 46 per cent, that is, the variability in mental test abilities is almost twice as great granted the existing differences in environment than would be expected if the only differences were in heredity. The straightforward averaging of coefficients of correlation differing rather widely in magnitude is highly questionable; and Willoughby apparently did not anticipate the criticisms of Fisher's formula which Burks and Hogben made subsequently.

Another study of physical traits, of interest because it utilized correlations from different kinds of twins, is that of Dahlberg (7). He reasoned that differences found in repeated measurement of the same person are due to errors of measurement; differences between monozygotic twins are due to errors of measurement plus the variance due to differences in environment; differences between same-sexed dizygotic twins are due to errors of measurement plus variance due to environment plus variance due to differences in heredity; and differences between opposite-sexed dizygotic twins are due to errors of measurement plus environment plus heredity plus sex factors. On the basis of an additive assumption concerning the contributions of these separate factors he thus was able to estimate the variability of the separate factors, error, environment, heredity, and sex. Unlike other investigators, Dahlberg expressed the variances of the factors as percentages of the original measurements rather than as percentages of the original trait variance. The procedure is a legitimate one because of the obviousness of the absolute zero for physical measures.

Unique for the fact that it does not make use of the coefficients of resemblance for relatives is the study of Kelley (16). He assumed that the deviation score of an individual on a given trait could be expressed as the sum of his deviation scores in a nature factor and a nurture factor.

> The two were differentiated by means of the supposition that the deviation from the age norm for the nature factor remains constant, while the nurture factor is weighted proportionally to the time through which it has acted on a growing ability. Recognizing the importance of the unit of measurement in assigning proportional values, he substituted "sensed differences" for the original score units. While standard deviation scores for all traits would have a variance of unity, when the "sensed difference" deviations are expressed in terms of the standard deviations, the variabilities of the traits will differ, and it is these variabilities from which Kelley obtained the degree of "idiosyncrasy" for any trait relative to any other trait. By comparing average groups of different ages, during which time the traits studied were supposed to be developing, the rela-

tive contributions of nature and nurture to the idiosyncrasy of a pair of traits was determined, and it was also determined whether environment exerted a leveling or a differentiating effect during the period studied.

Despite the elaborate conclusions arrived at on this basis, the study has not often been quoted in subsequent papers in this field. Perhaps its most serious weaknesses are the use of the unconvincing "sensed difference" units, the assumption of a constant nature factor, and the assumption, criticized by Burks (1), that the effect of environment increases directly with time.

The bulk of the discussion of the relative effects of nature and nurture on intelligence has centered around the data from studies of foster children and twins. Each type of study has claimed to achieve some degree of experimental isolation of heredity and environment. If the placement of children in foster homes of different quality is random with respect to the intelligence of the children, then the correlation between child's IQ and quality of home or foster parents' IQ is said to be due entirely to environmental influences on mental growth, insofar as the IQ test measures mental growth. Correlation between child's IQ and home factors for children raised in their own parents' home is said to be due to the influence of environment plus the influence of the inheritance of factors determining intelligence. The logic of twin studies is similar, as shown above for the data of Dahlberg. An additional type of twin comparison has been introduced, namely, a comparison of the resemblance of identical twins reared together, in which case the environments are closely similar, with the resemblance of identical twins reared apart, in environments of varying degrees of dissimilarity. Criticisms of the effectiveness of the experimental isolation of hereditary and environmental factors will be referred to but not repeated in detail, since the present review concerns rather the statistical methodology.

Probably the most often quoted percentages for hereditary and environmental contributions to intellectual variation are those of the California research of Burks (2), based on a comparison of foster children-environmental correlations with coefficients from a carefully matched control group of children raised in their own homes.

From nine separate measures of the intellectual value of the home environment, Burks selected those which would give approximately a maximal prediction of child's IQ in the foster and control groups. For 164 foster children, the multiple correlation was .35 between their IQ's and the best prediction on the basis of father's mental age, father's vocabu-

lary, mother's vocabulary, and family income. For the 95 control children, the multiple correlation was .53 between child's IQ and father's mental age, father's vocabulary, mother's mental age, and the Whittier Scale for Home Grading. Corrected for attenuation, these coefficients become .42 and .61.

Burks writes that "the multiple correlation corrected for attenuation (.42) is a measure in the Foster Group of the effect of home environment upon differences in children's intelligence. More precisely, the *square* of this multiple (.17) represents the portion of the variance of children in ordinary communities that is due to home environment." "In the Control Group, the square (.37) of the multiple correlation corrected for attenutation (.61) represents the *combined* effect of home environment and parental mental level upon the variance of children's intelligence." What of the 63 per cent of IQ variance not accounted for by home environment or parental IQ? The constancy of the IQ and the identity of identical twins argue against Fisher's suggestion that the remaining variance can be attributed to "random somatic effects of the environment." Because only one-half of the chromosomes of each parent are present in any offspring, Burks points out, the correlation with parental intelligence underestimates the correlation with hereditary determiners; she concludes that "probably the major share of the residual variance is due to congenital endowment." Thus, "close to 75 or 80 per cent of IQ variance is due to innate and heritable causes."

In a further analysis of the control group data, Burks utilized Wright's (40, 41) method of path coefficients.

The method of path coefficients assumes that a given variable considered as an effect is equal to the weighted sum of a set of other variables, considered as causes. On the basis of correlation between the effect, such causes as are accessible, and other related factors, the weights, known as path coefficients, are discovered. The squares of these weights are interpreted as the proportional contributions of the given causes to the variance of the effect. For "own" children, Burks set up child's IQ as the effect, with mid-parent intelligence and environment as measured by the Whittier index as causes.

She found that parental intelligence contributed 33 per cent of the variance in child's IQ, environment other than parental intelligence contributed less than one per cent, and the joint contribution of parental and environmental factors was three per cent. The sum of these percentages yields the square of the multiple correlation between child's IQ and the sum of parental intelligence

and environment as measured by the Whittier index. In this case, children raised in their own home, the contribution of parental intelligence occurs via both heredity and environment.

Wright (43) has criticized Burks' analysis of the causal relations, suggesting as a simple alternative that child's IQ is caused by heredity and environment, with parents' intelligence appearing as a variable correlated with the three other variables in the control group, but uncorrelated with heredity in the foster group. Correlations with parents' intelligence are used to solve for the unknown correlations with the postulated "heredity" factor. Solving the equations derived from this analysis of the causal relations, it is found that among the foster children home environment contributes nine per cent of the variance in intelligence, and the residual 91 per cent of the variance is assumed to be mostly hereditary. With the additional assumption that the ratio of the path coefficient of heredity to that of environment is the same for foster and own children, it is found that the proportion due to heredity among own children is .81, the proportion due to environment is .07 with the remainder of the variance due to the correlation of heredity and environment. From this analysis it is also found that the correlation between the child's heredity and the midparent intelligence is only .42, as compared with the obtained correlation of .86 between midparent intelligence and home environment. Wright points out that this discrepancy is not surprising in consideration of genetic theory. He also presents a more complicated analysis of the causal structure and presents the path coefficients obtained from this analysis.

Leahy (20) studied a group of foster children and a matched group of own children in Minnesota, taking even greater precautions than Burks had that there should not be a correlation between the heredity of the child and the grade of environment in which he was placed. For the children raised in their own homes she found the correlations between their IQ's and various environmental factors to be in the vicinity of .50, while for the adopted children the corresponding correlations were approximately .20. She obtained proportional contributions by squaring the correlation coefficients, concluding that "variance in intelligence is accounted for by variance in heredity and environment combined to the extent of about 25 per cent," in the control group, while in the adopted group "where environment is functioning independently of heredity, variance in intelligence is accounted for by variance in

environment only to the extent of about four per cent." While Leahy's analysis appears to leave 75 per cent of IQ variance unaccounted for, it should be noted that she did not utilize multiple correlations of IQ with environmental factors, that the proportion of variance attributed to heredity might be said to be too small because of the imperfect relation between such measures as parents' intelligence and the child's genotype, and that similarly the proportion of variance attributed to environment might be too small because of failure to measure significant aspects of the environment. The last criticism applies also to most other studies, but use of the multiple correlation technique, as in the California foster child study, is at least a serious attempt to meet the criticism.

A variation of the foregoing analysis was introduced by Burks (3), who used the data of her previous study and that of Leahy to estimate the relative contributions of nature and nurture to average differences between socio-economic classes, as defined by a modification of the Taussig occupational classification. In the California study four occupational classes were distinguished: professional; higher business and semi-professional; lower business; and skilled labor. The five grades of occupation represented in the Minnesota study were classified as: professional; business manager; skilled trades and clerical; semi-skilled; and slightly skilled and day labor. Unfortunately for the generality of the conclusions, the proportions of cases in the higher occupational categories were much greater than the corresponding proportions in the general population. The exposition of the analysis is among the more recondite in this at best obscure field; but the difficulty appears to come from the omission of steps rather than from erroneous inferences. The logic is somewhat as follows:

> Assume that child's IQ is the sum of the contributions of the total environment and of heredity, and that the correlation of parental occupational group with child's IQ depends on the correlation of occupation with the environmental and hereditary factors. Assume that the correlation between total environment and occupational group is the same for foster and control children, but that there is no correlation between heredity and foster-parental occupational group among the foster children. Assume that "the partial regression coefficients measuring the concrete effects of" heredity and environment on intelligence are the same for foster and control groups. Finally—and Burks fails to mention this—assume that the regression of child's IQ on father's occupational status is rectilinear. The predicted value of child's IQ for a given occupational class will be the mean value of IQ for that class. Using the formula

for rectilinear prediction, we find that the difference between the mean IQ for a given class and the total mean will equal the regression coefficient times the difference between the rank of the given occupational class and the average occupational rank. This regression coefficient is, as usual, the product of the correlation between child's IQ and occupational class times the ratio of the standard deviations of IQ and occupational class. For the control group the regression coefficient reflects the influence of both heredity and environment; for the foster group the regression coefficient represents the influence of the environment alone. It is then shown that on the basis of the foregoing assumptions, the regression coefficient for the controls can be expressed as the sum of a factor representing the influence of heredity and a factor representing the influence of environment, and that the latter is given exactly by the regression coefficient for the foster children. Thus "the difference in mean IQ of 'own' children grouped according to father's occupation is composed of two additive heredity and environment factors, the latter being given by the corresponding difference in group means of foster children." It is shown that the ratio of the standard deviation of foster group means to the standard deviation of control group means is a measure of the proportional contribution of environment, and the complement to one is an estimate of the proportional contribution of nature.

From her own study Burks arrives at the estimate 34 per cent of variance due to nurture, 66 per cent due to nature; from Leahy's study the estimates are nurture, 22 per cent; nature, 78 per cent. It is not entirely clear why a ratio of standard deviations is used to estimate the proportional contributions, rather than the more usual ratio of variances (standard deviations squared). If variances had been used, a considerably smaller proportion of causation would have been assigned to environment. The assumption of rectilinear regression of child's IQ on father's occupational category is neither particularly reasonable nor is it supported by the data used. The direction of the error thus introduced is not obvious. Burks (in a private communication) has defended the assumption of rectilinear regression on the grounds that the correlation scatters can be made linear by arbitrary choice of the numerical indices attached to the occupations. This consideration alters the assumption only slightly; it is assumed that the same arbitrary numerical indices which make the regression of child's intelligence on parental occupational group linear in the control group will make the corresponding regression linear in the foster group. By inspection of the group means in Table 1 of Burk's article, this assumption is questionable.

A method for analyzing the data from different kinds of twins to ascertain the proportional contributions of heredity and en-

vironment has been presented by Holzinger (13, 14) during the same period that the foster child studies were appearing, although the complete presentation of the Chicago twin study by Newman, Freeman, and Holzinger (25) did not occur until 1937. The variability of the difference between members of twin pairs was analyzed into the proportion due to heredity and that due to environment by a comparison of correlations between like-sexed fraternal twins and between identical twins, in each case limiting consideration to those raised in their own families. McNemar (23) has summarized the assumptions underlying the statistical analysis employed: "a. The trait variance for fraternals equals that for identicals; b. nuture influences are the same for both types of twins; c. differences due to nature are uncorrelated with differences due to nurture; d. the traits are not affected by the asymmetry mechanism; e. the variance due to errors of measurement is negligible." McNemar omitted mention of the basic assumption, namely, that the score of any of the individuals on any of the tests could be considered as the weighted sum of a hypothetical score on heredity and a hypothetical score on environment plus an error of measurement, in short, the same additive assumption that has been the basis for the preceding analyses. The authors conclude that the percentage contribution of heredity is from 75 to 90 for a variety of physical measures, and from 65 to 80 for several measures of intelligence. McNemar points out that measures of finger ridges were used in distinguishing identical from fraternal twins, thus spuriously increasing the differences between identicals and fraternals in this trait and consequently overestimating the effects of heredity. If we exclude finger ridges, the proportional contributions of heredity to physical measures ranges from .75 to .81. And taking into account the fact that the measures of intelligence are less reliable than the physical measures, McNemar shows that the percentage contribution of heredity to intelligence is of the same order of magnitude as its contribution to physical traits, using the statistical methods of Holzinger. The probable direction of errors in the other assumptions is examined both by the original authors and by McNemar, but the possibility of error in the fundamental additive assumption was not mentioned.

A further analysis involved correlating the IQ differences between members of the 19 pairs of separated identical twins with rated differences in their educational, social, and physical environments. The correlation of IQ differences with educational differences was .79, with social differences, .51, with physical differ-

ences, .30. The correlation between differences in educational and social environments was .26. From these values the authors arrive at "coefficients of determination," presumably comparable to the squares of path coefficients derived by Wright's technique: The difference between IQ's of members of separated identical pairs is 50 per cent attributable to educational differences, 10 per cent to social differences, 12 per cent to joint educational and social differences, 9 per cent to physical differences, 19 per cent to unknown causes. It is to be noted that these percentages are based on correlations between differences. Can we really be so confident that the difference between a pair of twins whose IQ's are 80 and 95 is exactly the same as the difference between a pair of twins whose IQ's are 150 and 165? A similar assumption of absolutely comparable units is made concerning rated differences in environment. McNemar calls attention to the fact that most of the correlations are not significantly different from zero, and that the differences between finger ridges correlates .52 with differences in IQ and .63 with differences in Educational Age, both correlations being greater than is required by the five per cent level of statistical significance. "From these values it might be argued that anything can happen with such a small sample, or that these 19 pairs are not all identicals, or that there is an undeniable relationship between finger ridges and mental ability." Or that the correlations based on differences between pairs are not legitimate for these data. The original authors have stressed that the greater differences found between separated identicals than between identicals reared together indicate that statements about proportional influence must specify the range of environment and the range of heredity involved.

Perhaps the first review of studies on the proportional influence of nature and nurture on intelligence is Kelley's (19) chapter in *Psychologies of 1930*. Emphasizing research which was recent at that time rather than a comprehensive survey of the literature, Kelley referred to three studies from the *Twenty-Seventh Yearbook of the National Society for the Study of Education*, those of Burks, Willoughby, and one by Thorndike (36) on the resemblance of siblings in intelligence. He included also such parts of the twin study by Newman, Freeman, and Holzinger as were then available. While specifically questioning the validity of the assumption that heredity and environment compose intelligence in an additive fashion, Kelley adopts this assumption because it is the basis for all discussions of proportional influence. Two portions of

his discussion are an addition to the present one. Thorndike had compared the correlation of .52, which Pearson found to be the average coefficient for the resemblance of siblings in some physical characteristics, with an estimated correlation of .60 for the resemblance of a corresponding group of siblings in intelligence. The difference, according to Thorndike, represents the influence on intellectual development of the environmental similarity occasioned by being raised in the same family. Kelley adds that if we are willing to consider the variance in intelligence as the sum of the variance due to heredity, that due to inter-family differences in environment, and that due to other causes, then the proportion due to inter-family differences in heredity is the difference between the squares of the two correlation coefficients, or nine per cent. He believes, however, that Thorndike's estimate of .60 as the sibling correlation for intelligence is too low, and that the intra-family differences in environment, which Thorndike did not emphasize, are as important in intellectual development as the differences between families. On the basis of these considerations, he suggests 44 per cent as a fairer estimate of the proportionate influence of nurture than the nine per cent implied in Thorndike's discussion. Kelley does not claim much finality for his estimate:

> Unfortunately neither in the case of the 44 per cent mentioned above nor of the 46 per cent derived by Willoughby do we have even approximate probable errors, not to mention systematic errors which may be more serious. I would imagine them to be large in both instances. The closeness of the two figures should be considered a coincidence and not an experimentally established agreement.

In support of the last statement it might be added that the two estimates are derived on the basis of contradictory assumptions: Thorndike assumed that environment acted to raise the sibling correlation for intelligence as compared to physical traits, relatively unaffected by environment, while Fisher's formula, used by Willoughby, assumes that environment acts always to lower correlations between relatives.

While he does not refer in detail to his early study of the relative importance of nature and nurture, Kelley makes use of two of the same assumptions in drawing up a hypothetical table of "Hereditary and environmental contributions to the variance in intelligence of a homogeneous group composed of white children attending the public schools of America." These two assumptions are that the variance due to differences in heredity is constant from birth to middle age and that the variance due to differences in environ-

ment increases with age. In the table of hypothetical proportions, the variance due to environment is zero at birth and equal to the variance due to heredity at middle age. At the eighth year 17 per cent of variation in intelligence is due to environment, at the sixteenth year, 44 per cent. The figures in this table of hypothetical proportions are, Kelley feels, "reasonable from a priori considerations" and consistent with the findings of the other investigators whom he reviews. It is difficult to find any very direct confirmation, either in the data cited or in common experience, for Kelley's statement, included in the table, that the total variation in intelligence at maturity is about twice the total variation in intelligence at birth.

Reviewing both the twins and foster children studies, Shuttleworth (31) derives another estimate of the proportional contributions of nature and nurture. He uses Wright's method of path coefficients, objecting to the previous analysis by Burks on the grounds that she misinterpreted the significance of parental intelligence, and that of Wright on the grounds that he misquoted Burks' results and failed to notice that the foster and control groups were matched for environmental variability.

He begins by assuming that the true IQ deviation from the mean of the population can be expressed as the sum of the amount of the deviation due to hereditary differences, the amount due to intra-family environmental differences and accidental factors and the amount due to inter-family environmental differences. The intra-family environmental differences are assumed uncorrelated with inter-family environmental differences and with hereditary differences. The proportional contribution of intra-family environmental and accidental factors is first determined from Holzinger's data for the correlation of identical twins reared together, assuming that identical twins have exactly the same heredity. The percentage obtained, 3.3, is then used as the proportional contribution of intra-family environment and accidents to the IQ of the foster and control groups, thus assuming that the percentages are the same under these conditions and that the variability of the identical twins was about that of the children in Burks' study. Assuming then that the multiple correlation of .42 between the IQ's in Burks' foster group and the measured environmental factors represented the true correlation between IQ and environment, it follows that the percentage contribution of inter-family environmental differences is 17.6, and the remainder, or 79 per cent, must be due to hereditary differences. Since the control group was matched with the foster group for IQ variability and also environmental variability, and since the proportional contribution of environment is a function of the ratio of the two variances, the proportional contribution of inter-family environment is the same for foster and control children, namely, 17.6 per cent.

It is then shown that in the control children 63 per cent of the IQ variance should be attributed to hereditary differences, whereas the remaining 16 per cent is due to the correlation of heredity and inter-family environmental differences. But, says Shuttleworth, the foster and control groups, matched for variability in IQ and environment, more logically might have been matched for variability in heredity and environment; so he corrects the percentages to approximately what they would have been if the (inferred) variability of heredity in the foster group were lowered to match the (inferred) variability of heredity in the control group, or that of the control group were raised to match the foster group. The percentages vary slightly from those of the original solution. Summarizing the results, Shuttleworth says,

> For California children of non-Hebrew, North European stock reared in the homes of their true parents . . . from 62.6 per cent to 66.0 per cent of the variance of individual differences in IQ is attributed to hereditary differences; from 2.6 to 3.3 per cent of the variance is attributed to accidents and intra-family environmental differences or to factors which make the environment of two children reared in the same family somewhat different, from 13.9 per cent to 17.6 per cent to inter-family environmental differences or to such factors as parental intelligence, family income, and cultural status of the home; and from 16.0 per cent to 20.9 per cent to the joint contribution of hereditary and inter-family environmental differences or to the correlation between endowments and environments.

The data of Leahy were referred to only briefly, as roughly confirmatory of the findings of Burks, but suggesting a higher correlation between heredity and environment.

Woodworth (39) recently has reviewed the most important of the foregoing studies and in some cases has introduced his own statistical analysis of the results. In evaluating the data of Newman, Freeman, and Holzinger, he arrives at the proportional contribution of environment by dividing the average difference between members of identical pairs reared together by the corresponding average difference for fraternals. When account is taken of the error of measurement in the IQ scores,* it is seen

* In his monograph Professor Woodworth does not explain how correction for the error of measurement is made. In reply to a request for information on this topic, he showed that the correction assumed (1) that the true difference between a pair of twins is uncorrelated with the difference due to error, and (2) that the ratio of the mean deviation to the standard deviation is constant for the distributions of true differences, of differences due to error, and of obtained intra-pair differences. Both conditions must hold for the fraternals and for the identicals

that environment produces approximately 40 per cent of the intra-pair differences for height, weight, and intelligence alike. Similarly, for the intra-pair correlations in the same three traits, the identicals show a higher coefficient of resemblance in all three; when allowance is made for attenuation due to the unreliability of the IQ measurement, "the data afford no ground for making any distinction between the three traits, as concerns the shares of heredity and environment." In reviewing Burks' study, Woodworth follows her in using the square of the correlation coefficient as a measure of the percentage variance "accounted for" by a given set of factors.

From the correlation of .61 between child's IQ and a combination of parental and home ratings, he infers that "inter-family variation in heredity and environment combined accounts for $.61^2 = .37$ of the variance of own children." He does not follow Burks in attributing most of the remaining 63 per cent of variance to heredity, but refers it to whatever factors "operate within the same family to make siblings differ." Using the correlation of .42 between foster children's IQ and home ratings, he states that "the 18 per cent of IQ variance due to inter-family variation in environment (in the case of foster children) is directly comparable with the 37 per cent due to heredity and environment combined (in the own children)." The simple process of subtraction yields 19 per cent of variance due to inter-family differences in heredity, but here Woodworth has neglected the possibility of a correlation between good heredity and good environment among own children. From Leahy's study he took the correlation between child's IQ and the total home rating, based on occupation, parental education, and economic and cultural level. Squaring the correlations of .23 for foster children and of .53 for own children, "gives only 28 per cent of the total variance of own children attributable to the inter-family variation in heredity and environment combined, and only 5 per cent attributable to inter-family variation in environment alone, leaving 23 per cent due to inter-family variation in heredity." He points out that the discrepancy between these percentages and those from the California foster study may be due to failure to use multiple correlation. Since the total home rating is a composite, and multiple correlation is essentially correlation with an optimally weighted composite, the objection might be rephrased to say that Leahy's correlations were lowered by the inclusion of irrelevant home factors or by weighting too heavily factors which were unimportant in determining the intellectual stimulation of the home.

From these and other studies Woodworth concludes, "The most striking feature of these results is the small share that can be attributed to inter-family differences in environment. Not over a

reared together. Condition (2) will hold if the forms of the three distributions are alike. In particular, the assumption is valid if all the distributions are normal, and there is reason to believe that this is approximately true.

fifth, apparently, of the variance in intelligence of the general population can be attributed to differences in homes and neighborhoods acting as environmental factors."

III

Since 1928 the analyses of the proportional contributions of heredity and environment to intelligence have utilized mainly two techniques, Wright's method of path coefficients, and the square of the correlation coefficient. Prior to 1928 Fisher, Dahlberg, and Kelley used methods which were essentially ways of analyzing trait variance into proportions due to heredity and to environment, on the assumption that the trait measurement is a weighted sum of hypothetical measurements of heredity and of environment. Further assumptions, necessary to show the relevance of obtainable data to the weights to be assigned to heredity and environment in this hypothetical summation, differed in the studies of these three investigators, as did also the nature of the data used. The additive assumption, that is, the assumption that heredity and environment add up to make IQ, is common to the subsequent papers using path coefficients and the square of the correlation coefficient. This additive assumption has not always been stressed by the authors of papers analyzing experimental data, but it is to be found clearly in all the papers discussing primarily the methodology of analyzing causation.

Wright (41) has admitted that the fundamental additive assumption is a limitation to the use of the path coefficient technique. In one paper (40) he showed something of the general conditions under which the additive assumption would offer a sufficiently close approximation to the correct value of an effect which was actually made up of the *product* of two causes. He does not attempt to justify making such an additive assumption in general, and in actual applications of his technique (40, 43) he does not examine the appropriateness of additivity of causes.

Tryon (37) has discussed the interpretation of the coefficient of correlation between X and Y in terms of the degree of determination of Y by X.

> The degree of determination of Y by X may be defined either as the per cent of the correlation of Y with its complete composite (i.e., unity) which the correlation with X contributes, or as the per cent of the total variance of Y which the variance of X alone contributes. If we assume that Y is equal to X plus residual factors, then the square of the correlation between X and Y gives the percentage of determination of Y by X. In discussing applications of this formula, Tryon points out that given a

correlation, it is often far from obvious which variable is to be considered as determining the other one. A more general case is covered by the assumption that X and Y are each equal to a common variable C plus their respective residuals. Then the determination of Y by the common variable C, which might also be called the determination of Y by X, is given by the square of the correlation between Y and C, shown to equal the ratio of the square of the correlation between X and Y to the square of the correlation between X and C. For this case it is necessary to know the correlation of either X or Y with C in order to ascertain percentages of determination. If we may assume that X and Y are equally determined by C, then the correlation between X and Y is itself the percentage of determination of each by the common factor. Using a method developed by Spearman, it is possible to find the correlation between either X or Y and C by means of two other reference variables sharing the same common factor. Tryon offers several objections to Nygaard's (**28**) discussion of a percentage interpretation of the correlation coefficient; Tryon's paper also makes clear that we cannot, with Nygaard, equate the percentage of dependence of Y on X with the percentage of mutual dependence between X and Y. Interpretation of the correlation coefficient in terms of number of overlapping elements in X and Y is shown to be a special case of the foregoing, specifying the nature of the common and residual variables in X and Y.

Tryon shows that his usage of "degree of determination" corresponds to the square of Wright's path coefficient, which is what Wright also calls percentage of determination. Finally, Tryon points out the assumption implicit in his entire discussion, namely, that the variables X and Y are composed by adding the common factor to the factor specific to each variable. In justification of the additive assumption he quotes Spearman (**32**): "Surprise may be felt that the measurement . . . should so simply consist of merely the *sum* of these added together. . . . The answer to this question is that our proof has depended upon usage of Taylor's theorem, according to which all mathematical functions however complex can, in general, be expressed in the above simple additive form with some approximation."

Dunlap and Cureton (**8**) have discussed partial and semi-partial correlation, path coefficients, coefficients of determination and multiple regression equations as methods in the analysis of causation. They show that all of these techniques depend for their significance on an analysis of the variance of the effect into the proportions of variance due to the various causes. "The application of all forms of correlation to the solution of the problems of causation rests for its interpretation on the principle of the analysis of variance." This principle states that *if* the score on the dependent variable is the sum of scores on a set of independ-

ent variables, then the variance of the dependent variable is the sum of the variances of the independent variables.

Similarly, Monroe and Stuit (24) have said, "It is frequently desired to secure measures of the contributions of two or more causes to a given effect or dependent variable. A basis for accomplishing this is an algebraic equation which expresses the dependent variable as a linear function of the independent variables given as causes." It is not necessary that the "independent" causes be uncorrelated. As justification for the additive assumption which underlies all interpretation in terms of causes, they quote Kelley's (17) fundamental linear theorem. Their discussion includes the correlation coefficient, variance ratio, partial and multiple correlation, path coefficients, and factor analysis. The passage of Kelley's to which they refer is essentially the same as the quotation from Spearman, invoking Taylor's theorem as the justification for expressing scores from different tests in terms of a common factor plus a factor specific to the given test. Kelley puts rather mysterious qualifications on the use of the linear assumption; the trait must be one "such for example as computation ability, which grows in the single individual from time to time and changes from individual to individual in an essentially homogeneous group in a manner not markedly saltatory," and the mean value for the group must not be small with reference to the standard deviation. The relevance of these qualifications to the use of Taylor's series is not shown, and in particular, the importance of the magnitude of the mean is hard to see, in view of the fact that the scores are expressed in deviations from means in constructing Taylor's series.

While these reviews have not included Holzinger's technique for analyzing the data from different types of twins, it is clear from his own expositions that it is based on the same additive assumption. Like the other techniques, it uses correlations to obtain the variances of the hypothetical heredity and environment factors.

IV

It is rather remarkable that although all of the techniques for analyzing the proportional influences of heredity and environment on intelligence depend on an additive assumption, only one of the papers utilizing the techniques with actual data has contained an examination of the appropriateness of the assumption. The methodological papers on analyzing causation either have not

examined the assumption, or have justified its use in an equally special case, or have justified it by a desultory reference to Taylor's theorem. But there also have appeared papers criticizing the entire attempt to assign proportions to the contributions of heredity and environment to intelligence, and it may be supposed that these criticisms are directed essentially at the fundamental assumption.

Niles (**26**) criticized at length the method of path coefficients, stating, among other things, "The basic fallacy of the method appears to be the assumption that it is possible to set up *a priori* a comparatively simple graphic system which will truly represent the lines of action of several variables upon each other, and upon a common result." Wright (**42**) replied that while such a diagram of causation is not generally possible, his method was applicable where such a diagram could be drawn up, and, in addition, provided a means of checking the appropriateness of assumptions concerning causal relations. Niles (**27**) answered that the appropriateness of the assumptions of causal relations could be checked only where one already had the information which the method of path coefficients claimed to supply, in which case the method is useless.

Hogben (**12**) shows that correlation can be used to detect the existence of genetic differences for a given trait in a given population, but he questions the meaningfulness of Fisher's attempt to quantify the relative influence of genetic and non-genetic causes of variation.

> He cites the experiment of Krafka on the fruit fly *Drosophila*, demonstrating that the *form* of the curve expressing the dependence of number of eye facets on temperature at which the larvae are raised varies with the genetic constitution of the fly. That is, the amount of contribution of genetic differences to differences in number of facets depends on the environment in which the larvae are raised, and similarly, the amount of contribution of specified environmental differences to differences in number of facets depends on the genetic constitution of the larvae.

The percentage contribution of environment can be defined only as the amount of variation in the trait when heredity is constant divided by the amount of variation in the trait when both heredity and environment vary. Several of the authors reviewed above have been careful to point out that statements about proportional contributions must specify the amount of variation in heredity and environment in the population for which the proportions hold. But this qualification is not enough to rescue meaning for the proportions, for if the amount of variation in the trait

when heredity is constant depends on the particular constant value of heredity, we shall have more than one answer, and thus no answer, for the proportional contribution of environment. Again, Hogben says, "From an experimental standpoint, what do we mean by making the environment uniform? We can do so in an infinite number of ways, some tending to bring out genetic differences which were not previously measurable, others tending to obscure genetic differences which were measurable before. Has a balance sheet of nature and nurture any meaning in this sense, unless we assume that the variance of a population, if affected at all, is necessarily diminished when the environment is made more homogeneous?" That such an assumption is false can be established with a single example, and Hogben gives further findings on Drosophila showing a type of genetic difference which would be accentuated in a particular constant environment. Can we consider the constant environment to be an average of the environments within the experimental range? In the case of number of facets in fruit flies with one of the genes for "bar eyes" we know that the expression of hereditary differences depends on the temperature in which they are raised, and an average temperature has a definite significance. But an average value of environment "would not have an equally definite significance for the study of human populations exposed to an indefinitely large number of as yet unmeasurable and unidentified environmental differences. Nor would any balance sheet drawn up on such an assumption entitle us to set limits to changes which could be produced by controlling the environment."

Another British biologist taking an essentially similar point of view is Haldane (11), who has written, "The important point is to realize that the question of the relative importance of nature and nurture has no general answer, but that it has a very large number of particular answers."

Among psychologists, too, there have been critics of the causal analysis. Kelley (18), in commenting on Burks' (1) discussion of the use of partial and multiple correlation for analyzing nature and nurture, protests that one should not try to reach conclusions concerning such unmeasured, abstract entities as nature and nurture, but should be content to describe the properties of the correlations that can be observed.

Schwesinger (29), in analyzing the heredity-environment problem, also argues against seeking too general solutions:

> The problem of heredity and environment must therefore be thought of not only as specific to each characteristic and to each environment, but as specific to each individual in respect to each of his characteristics, and to each factor of his environment. . . . We cannot speak of a good or of a bad environment except in terms of a particular characteristic of a particular individual, and of particular environmental factors.

While these authors have criticized the meaningfulness of the "balance sheet of nature and nurture," none of them has shown explicity at what point the often careful reasoning of the discoverers of proportional contributions has been erroneous. It will be the contention of this paper that the error lies in the common additive assumption. This contention differs from the point of view of the quoted critics only in explicitness, for three of the authors cited have also criticized the additive assumption, although in somewhat different contexts than their criticisms of the meaningfulness of the problem. Schwesinger has cautioned that changing the variability of nature or nurture would change their proportional contributions. The criticism is commonly known, and not decisive, for a similar criticism could be made of any coefficient of correlation. Kelley (**19**) has recognized that statements of proportional contribution rest on an additive assumption; he believes that an assumption that heredity and environment are multiplied to produce the trait value is just as reasonable, but it does not permit the analysis of percentage causation. His own arguments were not so convincing but what he could state, in the same article, intuitive values for the proportional contributions of nature and nurture. Hogben questioned Holzinger's nature-nurture ratio, based on the comparison of identical and fraternal twins, because "In dealing with intellectual resemblances, we have to remember that the greater physical similarity of identical twins may be associated with a more restricted choice of environment." In the discussion of the validity of Holzinger's methodology, Hogben does not seem to distinguish clearly between the question of whether heredity and environment can be treated as additive and the question of the correlation between good heredity and good environment with respect to their distribution in the population.

V

When is it justifiable to make an assumption? The answer may differ with the problem at hand, but the following requirements seem quite general. (A) The assumption must mean something. (B) It may be shown to be approximately true on the basis of

evidence from other fields of discourse, or alternatively, (C) it must be possible to show that the error committed by making the assumption is not larger than a certain amount dictated by the uses for which the results are intended. Condition (C) requires that some results based on the assumption be susceptible of empirical proof. The burden of this discussion will be that the additive assumption concerning the causation of intelligence is ambiguous in meaning, is not supported by any known evidence, and leads to no results capable of empirical verification.

As two papers have referred to Taylor's theorem as a justification of the linear or additive assumption, we may begin by considering exactly what this theorem states.

Taylor's theorem says (10): Given $y=f(x)$, where x is expressed in terms of deviations from some mean value, then y can be expressed as the sum of an infinite power series of x, with coefficients which are essentially derivatives of $f(x)$ evaluated at the origin, provided that the remainder after n terms tends to zero as n increases. Note that the expansion of the function y as a power series of its independent variable x is by no means always valid; the validity of the expansion depends on the remainder tending to zero. In order to show that the remainder tends to zero, it is necessary to have an analytic expression for the original function, and this function must have derivatives of all orders. Spearman's claim that "all mathematical functions, however complex, can, in general," be expressed by Taylor's series, is certainly wrong. Mathematical functions do *not*, in general, possess derivatives of all orders. Furthermore, the assumption that y can be expressed as a linear function of x is even more restrictive, necessitating that a good approximation to the value of the whole series be obtainable from the constant term and the term containing the first power of x. Granted that the function can be expressed by Taylor's series, that is, that the remainder after n terms tends to zero, it by no means follows that the remainder after *one* term is negligible. The goodness of approximation afforded by the first term, that is, by a linear expression, depends on the requirements of the problem, on the variability of x about its origin, here taken as the mean, and on the nature of the original function of x. It is impossible to ascertain the goodness of the approximation without knowing something about the functional relation between y and x, just as it is impossible to know whether Taylor's theorem applies at all without some such knowledge.

In the nature-nurture discussion, IQ has been the dependent variable y and at least two independent variables have been assumed, heredity and environment. Sometimes there have been assumed two independent environment variables, intra-family and inter-family.

Taylor's theorem for functions of two variables, x and z, states that $y=f(x, z)$ can be expressed as the sum of an infinite series of polynomials

homogeneous in x and z, with coefficients which are essentially the partial derivatives of the function. The conditions under which the infinite series represents the function correspond to the condition of the remainder tending to zero. Again, in general the best linear function of x and z will not be a good approximation to the value of y. Assuming that the Taylor's expansion is valid, and assuming that the first terms in x and z afford some degree of approximation, the goodness of approximation depends on the requirements of the problem, on the nature of the function $f(x, z)$, and on the variation of x and z about their respective means. Taylor's series for a function of three variables is a corresponding extension of the series for two variables.

Is it legitimate to say that because IQ is a function of heredity and environment, Taylor's theorem justifies the expression of IQ as a sum of heredity and environment? Quite apparently not. Taylor's theorem refers to a mathematical function possessed of partial derivatives of all orders. We do not know any mathematical function which expresses the relation of IQ to nature and nurture. In fact, it is highly doubtful what is meant by "heredity" and "environment" in this context. It is by no means clear that there is a linear continuum of "mental heredity"; there is good reason to suppose that there is not an unambiguous scale of "mental environment," as the writer (22) has had occasion to discuss elsewhere. Heredity and environment are not single-valued mathematical variables; they are related to IQ, but not by means of any known mathematical function, simple or complex. Thus Taylor's theorem offers no justification for the additive assumption basic to the discussion of the proportional contributions of heredity and evironment to intelligence.

Let us assume, however, that it is possible to assign a numerical value to intellectual heredity, and that numerical values can be assigned to one or more aspects of environment as they pertain to intellectual development. Can we then say that IQ is even approximately the weighted *sum* of these numbers? To do so is equivalent to saying that the contribution of a given environment to IQ is the same no matter what heredity it acts upon. This direct implication of the additive assumption does not seem to have been recognized by most of the authors in this field, it has been argued against by some of those who have accepted the additive assumption, and most important, it is inconsistent with what we know of animal genetics and of clinical psychology.

In utilizing the path coefficient technique in the foster child study, Burks (2) wrote,

> This situation is a particularly favorable one for using the Wright technique, for the assumptions regarding causal relations are here at a minimum. It is only necessary to assume that parental intelligence and home environment affect the child's IQ, but that the child's IQ does not contribute to these. It is not necessary to make any assumption at all regarding a possible causal or interacting relationship between parental intelligence and environment; merely the known correlation between the two is sufficient.

Similarly, Shuttleworth (30), on the same page on which he explained the necessity for the linear assumption in his analysis, said, "No assumption needs to be made concerning the nature of the interaction of hereditary and environmental differences." But the linear, or additive, assumption is equivalent to a statement that there are no interactions between heredity and environment, as the term interaction is used in statistical discussions. What Shuttleworth and Burks probably meant to say was that their techniques involved no assumption about the nature of the relation between heredity and environment in terms of their distribution, or correlation, in the population. The distinction is subtle, but crucial: The issue is not whether heredity and environment are distributed randomly with respect to each other, but whether a given grade of environment produces a given IQ increment regardless of the grade of heredity raised in that environment.

Burks (1), in a publication simultaneous with the foster child study, emphasized with italics the remark, "Environment may have different degrees of influence when the endowment for a given trait is of larger or smaller amount," which is a way of stating that there are interactions between heredity and environment. The data to which she referred concerned the inheritance of physical stature, and the results she quoted could be accounted for as well in terms of a mean rise in height uniform throughout the filial generation as in terms of the differential effect of environment on different heredities. Nonetheless, it is interesting that she apparently did not see that this idea, the interaction of heredity and environment, is in direct opposition to the additive assumption underlying the path coefficient technique. Thorndike (35) was probably expressing a similar criticism of earlier studies when he wrote, "Many of the false inferences about nature *versus* nurture are due to neglect of obvious facts," among which he included "that the problem of relative shares, where both are effective, includes all the separate problems of each kind of environment

acting with each kind of nature. Any one estimate for all cases would be absurd."

It is not easy to find explicit evidence of the interaction of heredity and environment in determining human traits, especially since the exact heredity and the relevant aspects of environment are difficult to ascertain. The example of the number of eye facets in Drosophila, quoted from Hogben, is an excellent illustration of the interaction of hereditary and environmental determiners. Both Hogben and Haldane give a large number of examples from general biology emphasizing that the effect of a given nurture depends on the nature on which it acts. It seems fully as plausible that this type of interaction is the general rule in development as to assume that there is no interaction.

Woodworth, while accepting Shuttleworth's use of the word interaction, appears also to be concerned with the problem as stated here since he adds a sharp distinction between the "objective environment" and the "effective environment." "Within the same home, then, two children may have very different effective environments for the reason, at bottom, that they differ genetically. Quite possibly this is the most important fact in the whole problem of heredity versus environment." The extent to which a child can choose his own level within a given environment will depend on the rigidity and limitations of the environment, he says. Thus the extent to which a child's mental development is influenced by intra-family differences depends on the nature of his mental heredity and on the nature of his family environment. The measurable aspects of environment, those which have been presumed to have a predictable effect on the intellectual growth of the child, are those which are ordinarily the same for two children in the same family. These aspects Woodworth calls the objective environment. The environmental differences within the family, modifying the effect of the measurable aspects, constitute what Woodworth calls the effective environment and substantially what Kelley and Shuttleworth have called intra-family differences in environment. Woodworth takes exception to the view of Burks—he might have added Wright and Leahy—that the variance in intelligence not shown to be due to measurable or inter-family differences in environment must be due to heredity. This view neglects the importance of differences within the family. Kelley has already been quoted as making a similar criticism of a statement of Thorndike's. From his own review of some of the

above and some other studies, Woodworth draws the significant conclusion, "The main causes of variation seem not to be such as differentiate one family from another in environment, or in heredity either. The causes, genetic and environmental, which make siblings differ seem to be more potent than those which differentiate one such family group from another."

Perhaps we can save the additive assumption, and consequently, the percentage analysis of causation, by postulating two environmental sources of variation, inter-family and intra-family differences. Attempts in this direction were made by Kelley (19) and Shuttleworth. Kelley simply decided that intra-family differences were as important as inter-family differences. Shuttleworth's method was even less acceptable. He supposed that the proportional effect of intra-family differences in environment could be estimated for children in general using the data from identical twins reared together, in whose case the heredity is presumably identical, and the environment much more nearly identical than for ordinary siblings. Surely the effect of intra-family differences would be far greater for children of opposite sex, of different birth orders, and for children who start with widely differing capacities and personality tendencies. Shuttleworth's estimate of three per cent of the variation in intelligence being due to intra-family variations in environment cannot be considered seriously, for it does not reflect the true extent of differences in family position, nor does it take care of the objection that a given measurable environment may have different effects on the intellectual development of children differing in mental endowment.

An acceptable estimate of the importance of the interaction of mental endowment and measurable environment in determining mental development is not given in any of the studies reviewed here. Is it likely that a statistical technique will be constructed, or a present technique modified, to enable us to estimate the significance of interactions? There is a very essential reason for the answer being negative. It must be remembered that in analyzing the proportional contributions to variance in intelligence, we do not deal with differences in mental heredity in terms of the great variety of mental qualities, but in terms of a single numerical index, and we do not deal with the environment in terms of its rich texture of subtle influences, but again only in terms of a single numerical index.

In order to apply statistical methods, we must have replication of a given set of conditions. In this case, replication implies that two children with heredity of, say, +.7 standard deviations and environment of, say, +1.2 standard deviations, would be considered exactly alike, not only in point of heredity and environment but also of interaction of heredity and environment. That is to say, it would be possible statistically to estimate the interaction of a given numerical heredity with a given numerical environment.

To do so by the methods now available would be necessarily to neglect those factors which we know from clinical psychology to be of greatest importance in the dynamics of human development, namely, the relationships of the child to the people and the circumstances of his milieu. We are far from knowing which personality factors in the child and in his parents and in his siblings are of maximum importance in mental growth. There is slight hope that the relationships and personality traits significant in the interaction of hereditary and environmental determiners of intelligence can be captured in one or two or half a dozen numerical indices; yet this is the condition for a percentage analysis of the causation of intelligence. While a study of the dynamic factors in the genesis of intelligence may not lead soon to a numerical evaluation of their relative importance, it will be of highest interest to psychological theories and quite possibly crucial to educational and clinical practice.

The really important criterion of the additive assumption is: how useful—and therefore how accurate—are the results obtained by making this assumption? The authors of some of the works reviewed have indeed drawn conclusions of wide social significance from their percentages. Kelley (16) found that for computation ability environment exerted a leveling influence, while it increased idiosyncrasy in history and literary information. "Should not our national educational institutions tend more strongly to a common level of achievement in History and Literature Information than towards such a level in Computation ability?" he inquires. For reading and history are "more intimately connected with the universal duties of citizenship," and literary and historical knowledge and aspiration are the meeting ground of alien people. The interests of democracy will be better served, he feels, by less equalization of computational ability and more equalization, say, of knowledge of history. But does not every citizen have to buy a loaf of bread? There is nothing compelling about his argument

that people ought to be relatively more alike in certain subjects than in others; a still graver lack is that he provides no way of finding out in the course of applying his conclusions concerning "idiosyncrasy" whether they are approximately correct.

Shuttleworth (30, 31) applies his results to the whole question of how best to improve the general intelligence of mankind. Admitting a certain number of broad guesses enter into the computations, he nevertheless feels it possible to make statements of the type, "The estimated increase in the average Stanford-Binet IQ is fifteen points for the past century and seven points for the next or a total indicated increase in the average IQ of twenty-two points." How shall such statements, even about the future, be tested? The content of IQ tests goes out of date, and when the tests are restandardized, the average will again be 100. How can we control such factors as widespread acquaintance with testing methods, which may very well influence the general level of response? Suppose we could solve these methodological problems. It is still doubtful that there could be a sensible answer to the question, "How far is it possible to raise the national IQ by improving the environment?" If it were found that the Stanford-Binet IQ is much influenced by environment, it might well be concluded, not that intelligence is influenced by environment, but that the Stanford-Binet is a poor test of intelligence because it is influenced by environment. McNemar (23) has said something very like this concerning tests of educational achievement, which have, after all, quite a high correlation with tests of intelligence. "The injection of educational achievement . . . introduces data which have little bearing on the classical problem as to the influence of nurture on tested ability, and therefore the unquestioned finding that achievement and personality are dependent partly upon environmental conditions cannot be accepted as of crucial importance in connection with the main problem." If it were found, on the other hand, that there is some measure of intelligence not influenced by environment, will we then cease to improve the environment? Certainly not, for it is an "unquestioned finding that achievement and personality are dependent partly upon environmental conditions." The "main problem," the "classical problem," to which McNemar refers, has a long history of elusiveness.

Not only is the classical problem of heredity *versus* environment still unsolved; some psychologists have questioned, from

various points of view whether it is the "main problem." In connection with recent studies on raising the IQ during the pre-school years, Burks (4) has commented that it is not necessarily desirable to stimulate the maximal mental growth during this period. The pre-school years have been shown to be extremely important in the formation and integration of the child's personal and social attitudes, and Burks suggests as more important than simply raising the IQ "that the growth of adaptive behavior should be stimulated and guided—often in special areas instead of all areas—so as to achieve a harmonious balance with the expanding personality needs of each particular child." An interesting possibility for the study of crucial factors in the development of adaptive behavior as a whole is exemplified in Burks' (5) detailed clinical study of a pair of identical twins reared apart. Line and Kaplan (21), also in connection with the influence of nursery school in raising the IQ, offer reason to believe that such a rise as has been observed can be accounted for by a social adjustment factor, whose acquisition is facilitated by pre-school attendance. They urge the evaluation of educational efficiency in terms of concepts with more explicit functional connotation than is possessed by "the IQ." Hogben has objected to the overemphasis on improving intelligence from another point of view: "To the writer it seems that the selfishness, apathy and prejudice which prevent intellectually gifted people from understanding the character of the present crisis in civilization is a far greater menace to the survival of culture than the prevalence of mental defect in the technical sense of the term."

It is the view of this paper that the validity of the additive assumption underlying the analysis of the causation of intelligence must be tested finally in terms of the empirical verification of the results of such analysis. The "practical applications" mentioned by the authors in this field are not rigorously deduced from the percentage analyses and do not afford a means of evaluating the correctness of the results in the course of applying them. Another way of approaching the problem of applications is to ask what problems need to be solved and then to inquire whether the results of the causal analysis can be of use in solving them. A very practical aspect of studying foster children has been mentioned by various authors, namely, that people who adopt children want to know in advance something about what the children are likely to become. The prospective foster parent will hardly want to

know the percentage of determination of the child's IQ by foster and own parentage; average values and dispersions and even correlations are more intelligible and more relevant than the final percentage statements about causation of differences. The rigorous exclusion of selective placement of children with respect to their probable intellectual levels, which is a part of the experimental isolation of the variables, is not necessarily the best way to obtain information concerning the most favorable placement policy. Another problem which some people believe to be of practical importance is the extent to which the prevalence of feeble-mindedness can be reduced by eugenic measures. Again, statements about percentage determination of intelligence are of little direct application; far more relevant would be studies exclusively of feeble-minded children and of their response to environmental improvements. The feeble-minded have, in fact, been excluded from the preceding studies. It appears that there are at present no practical applications or accessible consequences of the percentage analysis of the causation of intelligence in terms of which to test the accuracy of the various estimates.

Not much has been written recently on the numerical evaluation of the causes of intelligence, so that it may seem unimportant to have inquired so fully into the assumptions and utility of this line of research. It has been succeeded in popularity by research concerning the effects of nursery school on IQ, and many of the same considerations hold for these studies. Certainly "environment" means something more definite in the nursery school studies, but "the IQ" has become even more ambiguous because of the measurement difficulties peculiar to the pre-school years. It is still relevant to say, with Burks, that increasing the IQ is not the problem of central importance in the development of the child; with Line and Kaplan, that increasing the IQ is not the most essential problem from an educational view; and with Hogben, that increasing intelligence is not a focal problem from a social view. It may be said that whatever the practical importance of these heredity-environment studies, they are of theoretical importance. But the theoretical conclusions can be tested most convincingly by applications. If there exists a chasm between theoretical genetic psychology and applied genetic psychology, either we are not applying what we know, or we are not constructing theories about the most crucial aspects of human development. Indeed, one of the most fascinating junctures of theoretical

and applied psychology promises to lie in just that field, the dynamic interaction of heredity and environment, which was assumed not to exist in computing the "balance sheet of nature and nurture."

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PSYCHOLOGY AND THE WAR

Edited by
DONALD G. MARQUIS

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THE AVIATION PSYCHOLOGY PROGRAM OF THE ARMY AIR FORCES

STAFF OF THE PSYCHOLOGICAL BRANCH, OFFICE OF THE AIR SURGEON, HEADQUARTERS ARMY AIR FORCES

Washington, D. C.

The present article is the first in a series describing the Aviation Psychology Program of the Army Air Forces. This report treats the general organization and the historical development of this program up to October 1943. The last section deals with some of the recent and impending changes in the Aviation Psychology Program. Later articles will deal with specific aspects and areas of research.

1. The Psychological Testing Program for Air Crew*

Every man who applies for training as an Aviation Cadet is given a psychological examination to determine his general suitability for aircrew training. Men who score below the passing mark are not accepted. After entering the Army, the accepted applicant takes another examination to determine his educational background. The results of this educational examination are used for planning a five months' college training program. Finally each man is given a comprehensive series of classification tests at one of the Army Air Forces Classification Centers. These classification tests form the basis of an assessment of each man's proficiencies and special aptitudes for training as a pilot, bombardier, or navigator.

The purpose of this extensive use of psychological tests in the Army Air Forces is to select from the many applicants those men who are best fitted for aircrew training and to assign each of them to the type of training for which he is most suited. Through such a program the Army Air Forces secures a larger number of men who possess the psychological requirements for becoming first rate pilots, bombardiers, and navigators, and at the same time greatly improves the efficiency of the aircrew training program by reducing training wastage.

2. History of the Psychological Testing Program

During the interval between World War I and World War II, the problem of the selection of men to be trained as military pilots

* The first article dealing with the Aviation Psychology Program published in a psychological journal is: Flanagan, John C. The Selection and Classification Program for Aviation Cadets. *J. consult. Psychol.*, 1942, 6, 229–240.

was by no means without attention. In this country most of the research on aptitude tests for pilots was carried on by Flight Surgeons at the School of Aviation Medicine at Randolph Field, Texas. When the Air Forces began to train greatly increased numbers of pilots several years ago, it became increasingly important to develop objective psychological tests to be used in selecting men for this training. The interest of the Flight Surgeons in this problem made it logical to assign to the Office of the Air Surgeon responsibility for developing such tests. In June 1941, approval was given to the establishment of a psychological research agency in what is now the Office of the Air Surgeon. A small staff of professionally trained psychologists who were interested in the problems of Aviation Psychology was assembled. The initial work of this group was confined entirely to research. Psychological Research Units were established in each of the three centers of the Army Air Forces Training Command. Another research unit was established at the School of Aviation Medicine and later a fifth unit was established at Headquarters of the Training Command. In the fall of 1942, Psychological Research Detachments were sent to Army Air Forces Flexible Gunnery Schools.

The first psychological tests were given to Aviation Cadets in October 1941 at Maxwell Field, Alabama. Before deciding upon the psychological tests to be tried out for research purposes, extensive studies were made of all of the available information concerning the prediction of pilot aptitude. Conferences were held with officers of the Army Air Forces and with members of the National Research Council Committee on Selection and Training of Aircraft Pilots. Special studies were made of the training program of the Army Air Forces and of the reasons given for the elimination of cadets from flying training.

Soon after the research was undertaken it became evident that the program should be coordinated with similar research on the bombardier and the navigator. In December 1941, all psychological research of the Army Air Forces concerning pilots, bombardiers, and navigators was assigned to the Air Surgeon.

Before any of the original research projects were completed, the exigencies of the war made it necessary to change from a research program to a combined research and operating program. The first form of the Aviation Cadet Qualifying Examination was prepared and put into use early in 1942 for the original selection of applicants for Aviation Cadet training. Additional psychologi-

cal tests were selected for use at Army Air Forces Classification Centers for the purpose of determining the suitability of Cadets for various types of aircrew training. The tests that were used in this original program and the interpretation of the test results were based on an analysis of all the information available at that time and the judgment of the aviation psychologists on duty with the Air Forces. As research findings became available, refinements were made in tests and procedures. As a result, there has been a steady improvement in the selection and classification tests, as shown by the results that have been obtained in recent months.

At the end of the Fiscal Year 1943 (June 30) there were 85 Aviation Psychologists on duty in the Army Air Forces, all officers with professional training and experience in the field of psychology. Approximately eighty-five per cent of these men have advanced degrees in psychology. In addition, approximately 300 enlisted men were assigned to this program as Psychological Assistants. Practically all enlisted men have a college degree with a major in psychology or its equivalent. Thirty-five per cent have the Master's degree and six per cent the doctorate.

Lieutenant Colonel John C. Flanagan in the Office of the Air Surgeon, Headquarters Army Air Forces is the ranking Aviation Psychologist responsible for the technical supervision of the Aviation Psychology Program. In the original organization, Psychological Research Unit #1, Nashville, Tennessee, was directed by Lieutenant Colonel Laurance F. Shaffer. Psychological Research Unit #2, San Antonio, Texas, was under the direction of Lieutenant Colonel Robert T. Rock, Jr. Psychological Research Unit #3, Santa Ana, California, was directed by Lieutenant Colonel J. P. Guilford. Major Arthur W. Melton was in charge of the Department of Psychology, School of Aviation Medicine. Lieutenant Colonel Frank Geldard was chief of the Psychology Section, Office of the Surgeon, Headquarters, Army Air Forces Training Command. Later articles in this series will describe the organization, functions, and research accomplishments of these organizations, and the activities of recently established units.

In the Army Air Forces the Aviation Psychology Program has been the responsibility of Major General David N. W. Grant, the Air Surgeon, and Colonel Loyd E. Griffis, Chief of his Research Division. Brigadier General Eugen G. Reinartz is Commandant of the School of Aviation Medicine and Brigadier General Charles R. Glenn is Surgeon for the Army Air Forces Training Command.

3. Outline of the Aviation Cadet Selection and Classification Program

Aviation Cadets are selected from men who are already in the Army and from civilians. Application is first made to one of several

hundred Aviation Cadet Examining Boards located throughout the country. The applicant must be between 18 and 26 years of age inclusive. Recently, a special program has been opened to 17-year-old men who are placed in the enlisted reserve until they are 18. At the Aviation Cadet Examining Board, the applicant must show evidence of officer qualifications. He is given the Aviation Cadet Qualifying Examination and a special physical examination for flying duty. Men who successfully pass these examinations are accepted as applicants for aircrew training, but are not assigned as Aviation Cadets until a later time. The training program calls for a period of basic military training followed by five months in a college or university. Following this, under the plan in effect prior to October 1943, the individual was sent to a Classification Center where he was given the Psychological Classification Tests and a final type physical examination, his preferences were determined, and he was assigned to a specific type of training. The newly appointed Aviation Cadet next undergoes further training in a pre-flight school, and then enters upon his pilot, bombardier, or navigator training.

4. The Psychological Aptitude Tests

A. The Aviation Cadet Qualifying Examination. The initial psychological selection test has been developed for ease of administration by individuals who are not professional psychologists. The first eight forms are power tests for which a very liberal working time is allowed. Examinations contain 150 five-choice items and 6 different sections. Answers are recorded on a special answer sheet for machine scoring. Up to the end of the fiscal year 1943, eight different forms of the Aviation Cadet Qualifying Examination were released. Each new form represents an improvement over previous forms, as changes were made on the basis of accumulated research findings. The ninth and tenth forms of the examination, released recently, have been changed somewhat to allow for the inclusion of new types of materials that have been found to be of value. The type of material included in the examination and the length of each part have been adjusted so that the total score on the examination will have the highest possible validity. Because the majority of men who are accepted as Aviation Cadets are given pilot training, the examination is primarily a test of pilot aptitude.

B. Aviation Cadet Classification Tests. At the Army Air Forces Classification Centers, each cadet is given a series of approximately 20 different tests. The administration of these tests requires two days. On the first day the cadet takes various paper and pencil tests; on the second day he is examined with a number of apparatus tests.

A systematic effort has been made to develop for inclusion in the classification, battery tests for all of the skills or abilities that have been found to be important for the various aircrew duties. The types of tests that are now used can be divided arbitrarily into four general categories. Some of the tests appear to be fairly pure measures of specific abilities. Other tests appear to be more complex in nature. A general description of the different types of tests in use during 1943 up to October follows.

(1) *Tests of intelligence, judgment, and proficiency.* The classification battery included several tests of mathematical and numerical ability and a test of the ability to read and interpret technical information. In addition, certain tests were used to measure specific knowledge and information in some of the technical fields that are important for various aircrew specialties. Mechanical comprehension and mechanical information tests were included.

(2) *Tests of alertness, observation, and speed of perception.* The classification battery included tests which measured the ability to make accurate and rapid observations. These tests included questions pertaining to the perception of information contained in maps, photographs, tables, charts, and scales. All of these tests were constructed so that they contained material that was related to the perceptual problems that cadets meet in flying. Special studies were conducted concerning the scoring procedures for such tests, and the relative importance of speed and of accuracy was reflected in the use of speed and error scores.

(3) *Tests of personality, temperament, and interest.* The interests of the Aviation Cadets were assessed through the use of direct preference scales and the use of tests of information and other indications of active interests. One test was included in the classification battery that required the individual to perform a task without becoming confused, tense or unduly upset when he was faced with various distractions.

(4) *Tests of visual-motor coordination.* A number of psychomotor tests were used to test visual-motor coordination. Each cadet was examined on a separate piece of apparatus although the tests were set up in such a way that one examiner, operating a central control table, could test a group of four men. The timing of all tests and the recording of scores was effected automatically and objectively through use of a central control table. The classification battery included six different apparatus tests measuring visual-motor coordination and motor skill.

5. The Classification of Aviation Cadets

At the completion of classification testing, three scores are determined for each cadet. These are a Pilot Aptitude Score, a Bombardier Aptitude Score, and a Navigator Aptitude Score.

Each of the Aptitude Scores is determined by a procedure whereby each test is given a weight proportional to the contribution that it makes to the prediction of success in training, and an over-all weighted average of all the test scores is then obtained. Tests that do not contribute to prediction of success in a given specialty are, of course, weighted zero. Thus, for example, the Pilot Aptitude Score is determined from 10 of the 20 tests that each cadet takes, and each test that does contribute to the prediction of pilot success is given a different weight depending upon its contribution to the over-all prediction. Certain tests are weighted for more than one aircrew specialty. The manner in which this weighting procedure is actually accomplished is by recording all the scores on an aggregate weighting sheet and scoring it in an IBM test scoring machine having an aggregate weighting attachment.

Each cadet is given an opportunity to rate his interest in the different training specialties and to check one of four options that indicate whether he prefers to be classified for the training for which he has highest aptitude as indicated by the tests, or whether he wants greater consideration paid to his preference than to his aptitude ratings. On the basis of these data, the Psychological Research Unit recommends a specific type of training for each cadet. When the individual fails to meet the minimum psychological standards required for any of the three Aviation Cadet aircrew training courses, the recommendation is for enlisted aircrew or ground crew training. These recommendations are turned over to the surgeon at each classification center who has the results of a further physical examination and a standardized psychiatric interview. Final classification of Aviation Cadets is based upon the findings of the Psychological Unit and the Surgeon, and upon consideration of quota requirements. The final assignment is made by a board of officers.

The majority of Aviation Cadets prefer to become pilots. The number of men trained as pilots greatly exceeds the number trained for the other aircrew specialties so that it is possible in the majority of cases to assign men to the type of training for which

they have highest preference. The psychological aptitude scores are of special significance for the elimination of men who have very low aptitude for all types of training, and for the assignment of men who have decided differences in abilities as between the different specialties. Frequently, individuals desire to be recommended for the type of training for which they show the greatest aptitude on the psychological tests. In cases where preferences and Aptitude Scores conflict, the candidate is interviewed.

6. The Research Program

Although a major portion of the time and energy of the personnel of the Psychological Research Units in the Army Air Forces is devoted to the day-to-day routine of administering and scoring tests, maintaining records, and determining aptitude scores, it is recognized that present procedures can be improved and that extensive research is urgently needed for the continued growth and improvement of the program. The types of research problems which arise in the testing program and the general responsibilities and policies for conducting research are described in the following sections. These sections describe the program as it has developed up to October 1943.

A. Analysis of duties and functions. The research program involves first of all continuing studies of the duties involved in all the different aircrew assignments and of the conditions under which they must be performed. These job analyses are a necessary first step in analyzing the psychological functions involved in the different aircrew positions. Studies of the conditions of combat aviation are particularly important at this time. From an analysis of the psychological functions involved in operational and combat flying there are developed hypotheses as to the types of tests which should be significant in selecting men for combat.

Research in connection with job analyses has been carried on at all of the units concerned with the Air Forces testing program. Major responsibility for carrying on this work was assigned to the Field Studies Unit, Psychological Section, Research Division, Office of the Surgeon, Headquarters Training Command.

B. Modification or invention of testing procedures. As revised schedules of behavioral characteristics to be tested are set up, the research effort of the staff turns to the modification or invention of testing procedures which will give an objective evaluation of

those characteristics. Test ideas are welcomed, and do come from all the units—both from officers and enlisted personnel. However, in order to guarantee coverage of different aspects of behavior and to minimize overlapping, each unit was assigned primary responsibility for conceiving and developing tests of certain aspects of human behavior. The Psychological Branch, Office of the Air Surgeon, Headquarters Army Air Forces directed, supervised and coordinated this research. The assignment of responsibility was as follows:

Tests of personality, temperament, and interest
Psychological Research Unit #1, Air Force Classification Center, Nashville, Tennessee
Tests of coordination and visual-motor skill
Responsibility for research and validation:
Psychological Research Unit #2, Air Force Classification Center, San Antonio Aviation Cadet Center, San Antonio, Texas
Responsibility for research including design, procurement, and construction of apparatus:
Department of Psychology, Research Section, School of Aviation Medicine, Randolph Field, Texas
Tests of intelligence, judgment, and proficiency
Psychological Research Unit #3, Air Force Classification Center, Santa Ana Army Air Base, Santa Ana, California
Tests of alertness, observation, and perceptual speed
Psychological Section, Office of the Surgeon, Headquarters, Army Air Forces Training Command, Fort Worth, Texas
Aviation Cadet Qualifying Examination
Psychological Branch, Research Division, Office of the Air Surgeon, Headquarters, Army Air Forces, Washington, D. C.

C. Refinement and improvement of tests. From a clever test idea to a usable and validated test is a long and tedious road. Much of the research effort of the psychological organization is devoted to preparing test items, designing apparatus, trying out embryonic test forms on an experimental basis, revising and redesigning, gathering data for item analyses, determining reliabilities, and all the other routines of test construction which are involved in making a workmanlike and efficient test. Responsibility for this further development is assigned to those same units which have responsibility for originating the tests. Approximately 200 tests have been tried out experimentally thus far in the research program. Many of these were developed by personnel within the psychological program; others were modified to meet the needs of this organization.

D. Validation of tests. The final proof of the testing procedures which are being developed is, of course, their validity in terms of an adequate criterion of success. As is usual in practical testing problems, validation presents a difficult problem. The difficulty is in part due to the time that must elapse between testing and comparison of test scores with proficiency ratings after many months of training. Eventually, rather satisfactory data against which to validate test scores become available in the form of records of success or failure in training and of achievement in specific operational tasks. However, this information is not available until many months after the time candidates are tested for classification purposes. With the urgent need to improve selection and classification procedures as rapidly as possible, this lag represents a major problem in the improvement of the testing program.

E. Determination of efficient test batteries. In addition to the validities of individual tests, intercorrelations must be computed. From these, the best combination of tests may be determined and regression equations may be obtained for maximum prediction of success in the various aircrew positions.

Statistical analyses in connection with validation and with the determination of intercorrelations have been assigned to the Psychological Section, Office of the Surgeon, Headquarters, Army Air Forces Training Command.

7. Modifications of the Selection and Classification Procedures

As indicated previously, both the Qualifying Examination and the tests in the classification battery are subject to modification and revision in the light of results obtained. Additional tests may also be added to the battery as job analyses of operational flying and further research indicate new aspects of behavior to be tested. For example, the Qualifying Examination has recently been altered in the direction of including a speeded section involving problems related to the perceptual judgments important in flying. The apparatus tests are also modified and altered in the direction of increasing the prediction of aircrew performance. For example, research is underway at the present time on the addition of distracting and disturbing stimuli to certain of the psychomotor tests with a view to making the tests "stress" situations. New types of tests are being developed. For example, a number of moving picture tests have been tried out.

The procedures for recommending applicants for the various types of training are also examined regularly as additional evidence becomes available. Thus the standards of acceptance for pilot, bombardier, and navigator have recently been raised as validation data indicated that such a change was desirable.

Continual revision and refinement has increased the predictive capacity of the classification battery and of the Qualifying Examination to a considerable extent. At the present time, the degree of success obtained in predicting achievement in the various aircrew positions is as good as the best predictions that can be made of academic success in the colleges and universities of this country.

The above statement describes the psychological program as it operated during 1942 and most of 1943. At the present time a number of changes are being made in line with general War Department policy and in the interests of efficiency of operations and procedures. The various operating functions performed in the Psychological Branch in Headquarters, Army Air Forces are being transferred to the field units. The Psychological Branch in Washington will in the future be concerned chiefly with policy making, liaison, and general supervision of the program. The construction of the Aviation Cadet Qualifying Examination and the direct supervision of test development and the analysis of validation data has been delegated to the Training Command.

Another major change which is being made is to transfer the classification testing to the basic training centers. In the future all men going to the colleges from the basic training centers will have taken the complete battery of classification tests and have been found qualified for either bombardier, navigator, or pilot training. This involves the establishment of seven new psychological units. A Psychological Research Detachment (Gunnery) has also been activated, bringing the total number of units in the Training Command to eleven. The present directors and locations of these psychological units are as follows:

Major William M. Lepley
Army Air Forces Classification Center
Nashville, Tennessee

Captain Meredith P. Crawford
Army Air Forces Classification Center
San Antonio, Texas

Captain Neil D. Warren
Army Air Forces Classification Center
Santa Ana, California

Major Anthony C. Tucker
Army Air Forces Basic Training Center
Miami Beach, Florida

Captain Philip H. DuBois
Army Air Forces Basic Training Center
Jefferson Barracks, Missouri

Major Lewis B. Ward
Army Air Forces Basic Training Center
Greensboro, North Carolina

Captain William E. Walton
Army Air Forces Basic Training Center
Amarillo, Texas

Captain Clarence W. Brown
Army Air Forces Basic Training Center
Buckley Field, Denver, Colorado

Major Frederic Wickert
Army Air Forces Basic Training Center
Keesler Field, Biloxi, Mississippi

Major Merrill F. Roff
Army Air Forces Basic Training Center
Sheppard Field, Wichita Falls, Texas

Captain Nicholas Hobbs
Central Flexible Gunnery Instructors School
Buckingham Army Air Base, Fort Myers, Florida

Very recently Lieutenant Colonel Laurance F. Shaffer has been transferred to the Headquarters of the Redistribution Center, Atlantic City, New Jersey, to develop plans for psychological testing in connection with this program. It is proposed that a psychological research program be initiated at the Redistribution Center. This program involves proposals for testing and research on problems of selection and classification of air crew for combat duty, and investigation of the fitness of men returned from combat duty for additional service.

Present policy calls for increasing use of Aviation Psychologists in research on problems of training and on problems of later classification for special duty assignments.

As indicated previously, later articles in this series deal with the problems and procedures of specific research areas.

A PROGRAM FOR THE CLASSIFICATION AND TRAINING OF RETARDED SOLDIERS

BY LOUIS L. McQUITTY,* LT. COL., AGD
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The purpose of this paper is to outline the etiology, present status, and expected future development of a program of classification and training of retarded soldiers in effect as of July, 1943, at the Engineer Replacement Training Center, Fort Belvoir, Virginia.

Need for the Program

A very informal and unscheduled program of classification and training of retarded soldiers began shortly after the opening of the Center. The retarded soldiers sooner or later classified themselves by their inability to maintain the pace set by the other soldiers, and the instructors developed individual training techniques which they hoped would help bring the retarded up to the standard of the others. In an effort to achieve this goal, some of the instructors gave a disproportionate share of their time to individuals who, in general, were least qualified to profit from the extra instruction, and many of the techniques applied by the instructors were not the most effective. It soon became evident that both the retarded soldiers and the other soldiers would profit by an independent program for the retarded.

Etiology of the Training Program

The present training program for the retarded soldier passed through several stages of development. The initial stage involved gathering together into one company of approximately 200 men all of those trainees who had not been transferred out of the Center at the end of their training course, because they had been found to be unqualified in some way. The group included mental defectives such as feebleminded, psychopaths, psychotics, drug addicts, and physical defectives. This program was not entirely satisfactory. The company to which the retarded soldiers were assigned was one of a four-company battalion, and the morale and efficiency of the other three companies fell rather rapidly. It appeared that the composition of the company under discussion was a principal factor.

* On leave of absence from the Department of Psychology, University of Illinois.

The program was readjusted so that each of ten battalions was supposed to provide the specialized training for its retarded soldiers. This procedure was based on the theory that the morale of no battalion would suffer if each were alike and had to solve its own problems. This solution was found defective in at least two ways. The retarded, as a small group within a battalion, were made to feel too pointedly their deficiencies, and their morale was very low. Dispersal of the retarded throughout the Center, inherent in this program, made it difficult to achieve unified training for the men.

A better solution was found when barracks space became available for the group in an isolated company area several miles from the Center proper. The retarded soldiers have learned to regard this area as their own. Police is at a very high standard, and the men take a great deal of pride in their organization. Their morale is high. Many of the men transferred to this company soon find relief from their discouragement which was caused by competition beyond their abilities.

The first training program put in effect for this company assumed that all of the men should take approximately the same training. Experience soon proved to those concerned that there are wide ranges of individual differences even among the retarded. This is especially so in this case where the term *retarded* is applied to individuals who for any reason could not progress satisfactorily in the regular training program. It began to appear that there should be a program for each individual. A limiting factor in this respect was the available number of instructors. The next best effort appeared to be to divide the group into a number of homogeneous sub-groups and develop courses of instruction for the subgroups. In line with this principle, the following courses were developed.

1. *Elementary educational, military, and army engineering subjects.* This course is for illiterates and non-English speaking or writing soldiers. The illiterates and the non-English are under different instructors, and the educational subject matter is adapted to the respective needs of the two classes. The military and engineering subject matter is approximately the same, but is presented by the respective instructors in such a way as to adapt it to their classes.

Plans have been under consideration to include a mental therapy class. The class would have included maladjusted individuals who it was felt could be improved by group therapy. The class would have taken the same military and engineering subject matter as the two just mentioned classes, but would not have taken the educational subject matter. The

instructional time thus available would have been utilized in an effort to relieve the individuals of over-concern about their problems by attempting to interest them in the war through stories of war heroes, war atrocities, sufferings and the opportunities for service. Deterrents to the development of this class included scepticism as to its possible fruitfulness and the lack of qualified instructional personnel.

2. *Physical coordination and elementary military subjects.* This course is for those soldiers of low grade mentality who are particularly deficient in the ability to coordinate their muscular movements.

3. *Individual attention or observational course.* Soldiers about whom it is very questionable as to whether or not they can be developed into useful soldiers in a reasonable interval of time are transferred to this course, where they are given the advantage of more individual attention, in an effort to develop them so they can be of some usefulness. Despite this effort, the majority of these can not be so developed and are accordingly discharged.

The separation of these soldiers from the others for training has another advantage in that discharges require certain prerequisite administrative procedures. These are initiated when the soldiers are transferred into the course, so that they can be completed within a minimal time if discharge should be determined as the appropriate action.

The purpose of the above outlined three courses is to qualify as many retarded soldiers as possible for regular training and to determine as early as possible those who cannot be so qualified. All training is so scheduled as to make for frequent reviews. Every effort is made to teach by having the men do the thing being taught. All instruction is supposed to be on the concrete, objective level as contrasted with the abstract. Many training aids have been developed and are being utilized. A deterrent to the complete realization of some of these principles is the inability to obtain and retain qualified instructors. Individuals with the appropriate educational and experiential backgrounds are scarce among engineer soldiers, and those enlisted men who are qualified for instructors are usually of such a quality as to be approved for officer candidate schools. Qualified engineer officers, when found, usually advance to bigger assignments.

Etiology of the Classification Program

Helpful to any differential training program is a successful classification program. The present classification program for the retarded soldier developed gradually as did the training program. The classification problem was first realized as some of the soldiers fell behind in their training. Some of them, however, finished the regular training program before reports reached the Center Head-

quarters that they were not qualified for transfer to field units as trained replacements. They were held for additional study and training. Psychological and psychiatric officers were directed to study them. Some of the soldiers were discharged while others were given further training and transferred out to field units as replacements.

The studies revealed that many of the men had records in civilian life corresponding closely to their military records and suggested that the men could have been appropriately classified much earlier in the regular training program. The first attempt in this direction was to have soldiers who appeared to be retarded, in their first few days of training, appear before a board of officers. In searching for such men, particular attention was given to all who made low scores on the Army general learning ability test. The training instructors who selected men to appear before the board also furnished their reasons for the selections. Psychiatric and psychological officers, as well as troop officers, were on the board and appeared before the board as witnesses. The board procedure often demonstrated that the psychological and psychiatric officers had techniques and methods which enable them to arrive rapidly at what appeared to be valid judgments. Gradually the duties of the board were turned over to these specialized officers. The specialized officers developed a mental hygiene unit of psychiatric officer, psychological officer, psychiatric social worker and trained interviewers to carry on the duties previously accomplished by the board.

The Consultation Service

Considerable thought was given to the development of the mental hygiene unit with the hope that it could so function and be so interwoven within other units of the Center that no derogatory terms, or stigma, would develop concerning it. In line with this effort, it was decided to call the unit the "Consultation Service," this being chosen as a name which carries no derogatory implication but which still carries a true descriptive implication of the function of the unit. The unit was made an integral part of the Enlisted Division of the Center Headquarters.

The Enlisted Division is responsible for all the personnel administration, classification, and assignment of enlisted men of the Center. It is composed of nine principal sections, as follows: Pay Roll, Service Records, Special Problems (including duties in reference to furloughs, courts-martial, sale of bonds, dependency allotments, and sale of National

Service Life Insurance), Classification, Specialist Assignments, Consultation Service, Arrival of Troops, Special Order and Company Assignments, and Departure of Troops. In addition, a Dental Liaison Officer and a Chief of Dispensaries, or Medical Liaison Officer, both under the command of the Post Surgeon rather than the Replacement Training Center Commander, function at the Center in close cooperation with the sections of the Enlisted Division.

This organization ties the Consultation Service in closely with those other functions with which its duties are closely associated, facilitates cooperation, and encourages the Consultation Service to be accepted in the same manner as the other already established sections.

The internal organization of the Consultation Service requires dual responsibilities and qualifications of the psychologist. He is responsible for the proper psychological study of cases referred and the necessary army administrative procedure to effect the action found by the studies to be appropriate. He must qualify as an army administrator and as a personnel psychologist. The psychologist's worth is often judged by his army administrative ability rather than his psychological achievements. The psychologist should at least equal his administrative colleagues in the performance of administrative duties. If he does this, and usually only if he does this, his knowledge of psychology will enable him to be considered more valuable than his administrative colleagues.

The above points concerning the dual qualifications of the psychologist in the Consultation Service seem to suggest the possibility that psychology would be of much greater demand in similar situations in industry if our educational system trained students to be personnel managers, advertising specialists, production engineers and other like specialists, as it now does, and in addition gave them a thorough knowledge of psychology. This approach would probably produce students with qualifications and evaluations of their industrial responsibilities very similar to those which industry expects of young men employed as industrial psychologists. It would minimize the necessity of readjusting responsibilities and making up deficiencies in qualification now experienced by industrial psychologists who were trained principally by virtue of learning all the psychological aspects of industry. The young industrial psychologist might then be of more immediate value to industry and consequently in much greater demand.

The psychologist of the Consultation Service serves as a clearing house for all cases; he either takes final action on the basis of his own study or refers the case first to the psychiatrist for recommendations and then takes final action. Advantages inherent in

this plan of functioning are that one professional specialist is so trained as to facilitate putting professional recommendations into practice; while the other specialist, the psychiatrist, is left free to give practically all of his time to his professional duties.

Other advantages arise from the organization of the Consultation Service. These derive from the fact that the psychiatrist is on the staff of the Commanding General of the Center, rather than on the staff of the Post Surgeon. One of the advantages has to do with the fact that many cases appropriate to see the psychiatrist are out-patients and do not require hospitalization. Their appointments with the psychiatrist can be readily coordinated with the training program by the staff of the Commanding General of the Center; while such is difficult for the staff of the Surgeon, unless, of course, they clear through the staff of the Commanding General of the Center, thus causing more administrative work. The greater efficiency in coordinating the appointments with the training program encourages greater cooperation from the troop officers in making referrals.

Another advantage has to do with the fact that experience at the Center seems to indicate very strongly that the troop officers are more proficient in referring appropriate cases to the psychiatrist than are the hospital medical officers. The troop officers are in a much better position than the medical officers to observe the behavior on which the appropriateness of a referral must be based, as the troop officers and their noncommissioned officers come into daily contact, often under strenuous conditions, with the men. This suspected condition of greater efficiency on the part of the troop officers was a factor which motivated the development of the Consultation Service, where a psychiatrist would be available for referrals direct from the troop officers without the necessity of referring them first to a general medical officer. Another factor which motivated the development of the Consultation Service was the desirability of having a psychiatrist readily available so that he could lecture to the officers of the Center and hold informal talks with them, thereby improving their efficiency in referring appropriate cases.

The Surgeon General of the Army was favorably impressed by the way in which a psychiatric service had been interwoven into the personnel, classification, and assignment functions of the Center. He consequently made provisions whereby the Commanding Generals of other replacement training centers were offered the

opportunity to request psychiatrists on their staffs. Those requested are routinely placed on temporary duty at this Center to become acquainted with the organization and functioning of the Consultation Service before moving to their permanent assignments. Up to the present, fifteen psychiatrists have taken this temporary and informal training in the Consultation Service of the Center.

Soldiers are referred to the Consultation Service by training instructors, the military courts, the Provost Marshal, medical officers, Red Cross workers, and Chaplains. By far the majority are referred by the training instructors, and the majority of these are referred during the first ten days of training. It is the responsibility of all training instructors to observe and study very carefully all their men during this period, and especially those who made low scores on the general learning ability test, with a view to recommending for special development training all those who will profit by it and all of those who will not be of usefulness to the Service unless they do profit. Other appropriate cases which are not spotted during the first ten days are recommended for consultation whenever discovered. Included among these latter cases are drug addicts, constitutional psychopaths, those with abnormal reactions to some phase of training such as firing or demolitions, epileptics, disciplinary cases, and others who do not usually reveal their symptomatic traits until after the first ten days of training.

Each military court, after it officially closes a case, recommends whether or not the soldier should be referred to the Consultation Service. The convening authority of the court studies the report of the case and also recommends whether or not the soldier should be referred. All those recommended for consultation by either the court or the convening authority are seen, as well as some not so recommended if a review of the latter cases by the psychologist seems to indicate such to be desirable. After the soldier has been studied by the Consultation Service, the convening authority may readjust the sentence in the light of the report and recommendations of the Consultation Service.

A routine referral to the Consultation Service passes through the following steps:

1. An individual intelligence test.
2. An interview, covering (a) Educational achievements and rate of progress; (b) Occupational history and economic achievements; (c) Social adjustment; (d) interests; (e) Army adjustment; (f) General knowledge; and (g) Abilities.

3. Recommendations by the interviewer as to appropriate action.
4. A review by the psychological officer of the records accumulated in the above process; a re-interview by him if desirable; and a referral to the psychiatric officer for further study and final recommendations, if appropriate.

If the soldier is referred with the suggestion that he take special development training, the recommendation of the psychiatrist always includes a statement as to whether or not such training should be given.

In the case of soldiers suspected of questionable mental health, or who appear to be appropriate for discharge, referral is usually made to the Red Cross social psychiatric case worker who obtains a case history through Red Cross channels before final disposition.

Some of the soldiers have records of recent military misdemeanors. In these cases the psychiatrist makes one of the following findings:

1. Soldier is fully responsible for his behavior.
2. Soldier is not responsible for his behavior.
3. Soldier is responsible with the following mitigating conditions _____ and will or will not profit by punishment.

The policy is to punish those fully responsible, not to punish those not responsible, and to let the expected influence on other soldiers be the determining factor in the cases where the soldiers are responsible with mitigating circumstances and not expected to profit by punishment. If they are expected to profit by punishment, they are punished.

Some of the soldiers are returned to regular training, and others are transferred to special development training. In any case, suspenses are kept to assure return at the assigned time for further study, whenever such is appropriate.

The soldiers transferred to special development training are carefully observed and studied by their instructors who are required to keep records of just what the soldiers achieve or fail to achieve in their training. Near the end of each special development training course, the instructors recommend one of the following dispositions for each of their men: (a) discharge; (b) return to regular training; or (c) transfer to another course in special development training for improvement of other deficiencies.

The recommendations are reviewed by the psychological officer. At his discretion he re-interviews, approves, readjusts the recommendation, or refers the case to the psychiatric officer for a de-

cision. The psychiatric officer has the same three above-listed alternatives for disposition of each case referred.

Discharge proceedings are considered appropriate if it is felt that the soldier can be of no usefulness to the Service. This is assumed to be true just as soon as evidence reveals a history of, predisposition toward, or diagnosis of psychoneurosis, mental deficiency, epilepsy, constitutional psychopathic state, or psychosis. It is also found to be true in other cases which do not fit any formal category. Discharge proceedings are not decided as appropriate in these latter cases until attempts at special development training have indicated strongly that the soldier cannot be made useful to the Service.

The usual discharge procedure requires a board of at least three officers. They review the reports of the psychological officer, psychiatric officer, social psychiatric worker and instructors and may interrogate any of these as well as the soldier. The interrogation makes maximum use of questions designed to derive objective descriptions of behavior rather than interpretations or opinions by the witnesses and thus reduces the amount of conflicting evidence. The purpose is to give the board as complete a picture as possible, within the available time, of the behavior of the soldier which is closely related to the decision which it must make. This plan of interrogating the witnesses has proved so helpful in saving time, avoiding conflicting evidence, and facilitating decisions that a standardized group of questions has been prepared and is being utilized.

The board may recommend approval or disapproval of the discharge to the Commanding General, who is the final authority. If the discharge is disapproved, it may be recommended again after further training and study, or the soldier may be returned to regular training along with all others not discharged. He, as the others, may eventually graduate from regular training and thus be made ready to go forward as a trained replacement and do his part toward bringing this war to an early and successful end.

Future of the Programs

As this paper is being written, a plan is being completed for transferring some of the responsibility for special development training from Replacement Training Centers to Reception Centers. The plan includes special development training for all men scoring grade V, approximately the lowest seven percent, on the general

learning ability test. This is the group from which many of the candidates for the training come, and all of them will probably derive definite benefit from it. This plan avoids the necessity of starting all cases in regular training in order to determine which are going to do so poorly as to necessitate giving them the first priority in the limited facilities available at Replacement Training Centers for special development training. The new plan means that the individuals of low intelligence will arrive at Replacement Training Centers only after they have graduated from special development training. This should do much to enhance their chances of a successful adaptation and for this reason alone should prove superior to the present program.

SUMMARY

This paper shows how an instructional program for retarded individuals, and a mental hygiene program, both similar to those which are today found in many modern educational systems, developed in a Training Center of the Army. They developed as the execution of the regular training program demonstrated wide individual differences and as psychological and psychiatric officers made known special techniques and methods of handling these differences.

In developing the mental hygiene program, particular care was taken to tie it in closely with other personnel functions and to keep the psychologist well informed on army administration in order to prevent conspicuousness and false evaluations of the program. The efforts in this direction and the resultant high cooperation between this program and other functions of the Training Center have been so successful that the program is serving as a standard for the establishment of similar programs in many other training centers.

THE ARMY SPECIALIZED TRAINING PROGRAM COURSE IN PERSONNEL PSYCHOLOGY

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For the first time in history eleven departments of psychology are now giving the same series of courses, using the same subject matter outlines, to similarly selected groups of students all working toward the same objective. The courses are those included in the Personnel Psychology curriculum under the Army Specialized Training Program. The students are soldiers selected for training as personnel technicians in the Army of the United States.

During the fall of 1942 plans were made by the War Department to establish an Army Specialized Training Program early in 1943. In anticipation of this program, the Emergency Committee in Psychology of the National Research Council asked Dr. Walter Hunter to survey the existing supply of professionally trained psychologists and to collect estimates from the several branches of the federal government on the prospective 1943 demand for psychologists. A brief account (1) of Professor Hunter's findings showed a total supply of from 3,400 to 4,000 of whom about 25% were already serving as psychologists in the Army, the Navy, and civilian government agencies. In view of the prospective exhaustion of the civilian supply of psychologists (many of the remaining 75% were not suitable for military service because of age, sex, health, or other duties), psychology was included as one of the advanced courses under the Army Specialized Training Program (2).

The Army Specialized Training Division appointed a committee consisting of Walter Hunter; Major Karl M. Dallenbach, Office of the Adjutant General; L. L. Thurstone, Expert Consultant to the Secretary of War; and Leonard Carmichael, chairman of the Division of Anthropology and Psychology of the National Research Council, to outline a curriculum for a course in Personnel Psychology to last for two terms of 12 weeks each. The course was not intended to make professional psychologists of the men in that time, but was intended to convert carefully

selected soldiers who had had at least three courses in psychology and some work in college mathematics into acceptable personnel technicians.

The program adopted calls for 29 class hours of work per week throughout both terms. In addition to the academic work, the men are given 5 hours of military classes, 6 hours of physical exercise and 18 hours of supervised study each week—a program totalling 58 hours a week.

The courses covered in each term, the total weekly hours required and the recommended distribution between class and laboratory time, and a brief indication of the content of each course in the program are printed below.

FIRST TERM

Course	*Required Total hrs./wk.*	*Recommended hrs./wk.*	
		Class	*Lab.*
STATISTICS	9	4	5
(Tabulation, graphical methods, measures of central tendency and dispersion, standard scores, reliability, errors of estimation, correlation.)			
TESTS AND MEASUREMENTS	9	4	5
(The logic of measurement, psychophysical methods and their relation to measurement, history of measuring techniques, construction and use of various kinds of tests.)			
OCCUPATIONS AND VOCATIONAL PSYCHOLOGY	9	4	5
(Occupational groups, families, hierarchies, distributions and trends, relations between military and civilian jobs, study of representative jobs, the Dictionary of Occupational Titles, and army regulations on classification and assignment.)			
SOCIAL PSYCHOLOGY	2	2	0
(Attitudes, leadership, morale, public opinion, propaganda.)			

SECOND TERM

Course	Required Total hrs./wk.	Recommended hrs./wk. Class	Lab.
WORK, FATIGUE, AND EFFICIENCY (Fatigue and monotony, motor driving problems, accident prevention, industrial relations, efficiency.)	4	4	0
NORMAL AND ABNORMAL PERSONALITY (Psychological theories of personality, diagnosis, measurement, symptoms of mental disease, personality defects.)	2	2	0
PERSONNEL METHODS (Administrative personnel problems, job analysis and job specification, record keeping, time study.)	5	2	3
LEARNING AND PERCEPTION (Learning, conditioning, transfer of training, vision, hearing, organic sensitivity, acuity, camouflage, sound localization, perception.)	9	4	5
TESTS AND MEASUREMENTS: INTERVIEW METHODS (Continuation of tests and measurements course from 1st term with emphasis on interviewing methods.)	9	2	7

Groups of men for this training have been assigned to eleven schools. According to present plans, no additional groups will be started, either at these or at other schools. The schools, the number of men assigned to each, and the names of members of the faculty giving instruction in one or more of the courses are listed below arranged according to starting dates.

MAY 10, 1943

UNIVERSITY OF PENNSYLVANIA: Robert Brotemarkle, *Director;* 123 men. *Instructors:* F. P. Bakes, H. K. Brobst, R. A. Brotemarkle, J. C. Diggory, R. S. Driver, W. E. Fisher, F. W. Irwin, M. S. Murphy, M. G. Preston, R. C. Rogers, A. S. Thompson, and M. S. Viteles.

UNIVERSITY OF CHICAGO: Dael Wolfle, *Director;* 134 men. *Instructors:* A. W. Brown, J. B. Carroll, M. H. Groves, W. E. Henry, A. S. Householder, F. A. Kingsbury, L. Phillips, W. B. Stephens, F. Swineford, L. R. Tucker and D. L. Wolfle.

MAY 17, 1943

STANFORD UNIVERSITY: Paul Farnsworth, *Director;* 109 men. *Instructors:* R. G. Barker, G. E. Brown, Jr., R. K. Campbell, P. R. Farnsworth, M. M. James, R. F. Jarrett, G. A. Magaret, P. Mussen, G. Nettler, A. C. Sherriffs, C. P. Stone, E. K. Strong, Jr., C. E. Stuart, and R. C. Travis.

JUNE 14, 1943

CORNELL UNIVERSITY: H. P. Weld, *Director;* 151 men. *Instructors (first term only):* T. L. Bayne, M. E. Bitterman, J. Cohen, G. H. Smith, H. Palmer, T. A. Ryan, R. K. White, and A. L. Winsor.

HARVARD UNIVERSITY: Carroll C. Pratt, *Director;* 126 men. *Instructors:* G. W. Allport, J. G. Beebe-Center, R. B. Cattell, R. Dennett, S. G. Estes, E. A. Haggard, T. M. Harris, D. Horn, T. L. Kelley, C. R. Langmuir, R. H. Mathewson, G. A. Miller, O. H. Mowrer, C. C. Pratt, A. W. Purvis, D. C. Randall, D. W. Taylor, K. S. Wagoner, R. A. Waite, N. Y. Wessell, and R. W. White.

THE OHIO STATE UNIVERSITY: H. E. Burtt, *Director;* 122 men. *Instructors (first term only):* H. E. Burtt, F. C. Dockeray, H. B. English, C. Evans, R. P. Fischer, L. Hadley, J. Kinsella, F. N. Maxfield, R. Mooney, F. P. Robinson, H. A. Toops, and A. M. Wellington.

UNIVERSITY OF CALIFORNIA: Warner Brown, *Director;* 97 men. *Instructors:* O. L. Bridgman, W. Brown, E. Brunswik, R. H. Gundlach, J. R. Hamilton, H. H. Kelley, M. Levin, J. Loevinger, J. W. Macfarlane, R. N. Sanford, E. C. Tolman, and W. M. Wickham.

UNIVERSITY OF PITTSBURGH: J. Stanley Gray, *Director;* 121 men. *Instructors:* P. A. Brown, A. G. Dietze, L. M. Douglas, W. Garrett, O. E. Graebner, J. S. Gray, W. R. Grove, J. W. Harbaugh, G. E. Jones, J. S. Kinder, R. A. Patton, and W. Willis.

JULY 12, 1943

PURDUE UNIVERSITY: Joseph Tiffin, *Director;* 126 men. *Instructors (first term only):* E. J. Asher, D. C. Beier, I. B. Kelley, C. H. Lawshe, H. E. O'Shea, H. H. Remmers, G. A. Satter, W. R. Thompson, G. R. Thornton, J. Tiffin, and H. J. Vasconelles.

UNIVERSITY OF MINNESOTA: H. P. Longstaff, *Director;* 130 men. *Instructors (first term only):* J. E. Anderson, S. E. Baden, C. Bird, A. H. Brayfield, W. W. Cook, O. Cross, L. R. Harmon, D. B. Harris, W. T. Heron, H. P. Longstaff, K. MacCorquodale, D. G. Paterson, H. F. Roth, W. Schofield, I. Simos, and C. E. Stacey.

AUGUST 9, 1943

STATE UNIVERSITY OF IOWA: Kenneth W. Spence, *Director;* 120 men. *Instructors (first term only):* A. D. Annis, G. R. Bach, P. Blommers, C. E. Buxton, L. Festinger, C. d'A. Gerken, H. H. Kendler, N. C. Meier, H. V. Meredith, K. W. Spence, C. R. Strother, and J. B. Stroud.

The total number of men originally assigned to the eleven schools was approximately 1,350. Some of these have already

been dropped for academic failure or other reasons; others will be. It is quite specifically intended that the men be made to work hard and that the unsuccessful ones be eliminated. As an estimate, perhaps 1,000 men will receive certificates showing satisfactory completion of the program.

The quality of men selected for training was variable but generally excellent. Standards for selection were given to the STAR centers (Army installations which make the final selection of men for all ASTP courses) in terms of minimum Army General Classification Test (AGCT) score and previous college training in psychology and mathematics. As one example, nearly two-thirds of the men assigned to the University of Chicago were college graduates. The entire group averaged 134 on the AGCT. This score is 1.7 S.D. above the Army average. Even so, over 70 of the 134 men did not meet the technical requirements of three previous courses in psychology and one year of college mathematics. In most cases groups assigned to schools starting at later dates were more carefully selected than the men sent to Chicago. In some of these schools over three-fourths of the men were college graduates; for some the minimum AGCT score was 130. All of the Directors with whom I have talked have commented on the high average ability of the groups assigned to them.

On September 3, 1943 a one-day conference of directors and teachers of the program was held at the University of Chicago. The conference was attended by J. E. Anderson, University of Minnesota: C. E. Buxton, and K. W. Spence, State University of Iowa; C. Rogers, Ohio State University; J. S. Gray, University of Pittsburgh; J. B. Carroll, W. B. Stephens, L. R. Tucker, and D. L. Wolfle, University of Chicago; and E. R. Guthrie and D. G. Marquis of the A.P.A. Committee on the Graduate and Professional Training of Psychologists. The day was spent in discussing details of the program, methods and projects found effective in the different classes, and lessons which the instructors might learn from experience with the Army groups which should be applied to the teaching of regular classes.

The principal feature of the ASTP courses which most instructors wished to retain for regular classes was the rather large amount of class time spent in laboratory or supervised work periods in such subjects as statistics or tests and measurements. For both courses, the Army program called for four class and five laboratory hours a week. The instructors agreed that this schedule

resulted in a more thorough mastery of the material than does the usual individual study method and that the superiority was great enough to justify the added expense of conducting the extra supervised hours. The advantage of this method of instruction is one of the things most frequently commented on by the soldiers in the class at Chicago. In view of this agreement, the men present at the September 3 conference recommend the inclusion of work periods or periods of supervised study as part of the regularly scheduled program of courses such as statistics or tests and measurements.

A second point of agreement at the conference was the desirability of giving wider circulation to teaching materials and projects in psychology. A number of the projects described at the meeting by a man from one school are being or are going to be used at other schools. It was decided to request the director at each school to distribute copies of all available teaching materials to each of the other schools. In this way some of the particularly successful projects will become more widely useful both now and later. A policy of exchanging teaching materials somewhat as we now exchange reprints of articles could well be continued after the war.

An unfortunate feature of the program regularly commented upon whenever instructors get together from different schools is the large amount of overlapping between courses. The individual course outlines appear logical and consistent but overlap between courses becomes very obvious both to students and instructors when all students are taking the same set of closely related courses all at the same time. This overlap has been deleted from the sketchy outlines printed above, but in a number of cases it was unnecessarily great. The topic "morale" was listed in three separate courses; "tests and selection methods" was included in five of the courses. Most of the schools have minimized this overlap by various rearrangements of topics. In rearranging, however, the prescribed topics have been retained so that the work covered in one school is essentially the same as that in another. Such overlap also, of course, characterizes many civilian course outlines. In both cases it merely reflects our professional uncertainty and disagreement over the proper organization and classification of psychological content.

Several of the eleven schools have arranged to give college credit now for the work done in the course. Others plan to provide for such credit later where it is wanted through the offices of the

Armed Forces Institute. In view of the accelerated pace of the program, the amount of credit being given is greater than that earned by the average college student in an equal number of months.

It is hoped that the teaching of psychology will derive some lasting benefits from the experience with this course. By the time the last man graduates next February approximately 150 teachers will have participated in the program. Their experience with a course which differs in a number of respects both in method and in content from our traditional courses, the necessity of eliminating undesirable overlap in material, and the inevitable critical examination of psychological ideas, methods, and organization should result not only in the training of men for the Army, but also in the improvement of our civilian training of personnel technicians.

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THE INTRODUCTORY COURSE AND MILITARY PSYCHOLOGY

BY NORMAN C. MEIER

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The probability of the war's continuance in Europe into 1944 and indefinitely in the Far East sector argues for a reasonable adjustment of the college curriculum to fit the needs of students who will actually be in classrooms during the period. With few graduates, a reduced number of upperclassmen, and sophomores facing an early curtailment of their academic progress the chief need for adjustment would seem to be at this lower level. Adjustment may take any one or all of these directions:

1. Revision of the content of the Introductory Course.
2. Liberalizing of prerequisites and sequence requirements, making more courses available to sophomores.
3. Introduction of special wartime courses.

Revision of the elementary Course was proposed recently by Smith (12) and by a Committee on Psychology for the U. S. Office of Education (6). The latter proposes two types of course, one presenting general principles with applications and illustrative content having wartime reference, and one organized as a series of problems featuring motivation, morale, attitudes, propaganda, personnel problems, conflict, strain, adjustment and leadership. The recommendations are sufficiently flexible to permit some degree of modification. Other suggestions for revision of the Introductory Course have recently been made also by Ruja (11) and Wolfle (13), but these two, however, were advanced more with reference to normal times. At Iowa the plan has been adopted of revising the Introductory Course somewhat along the lines of the U.S.O.E. report but with a somewhat different envisagement of the problem. Inasmuch as the wartime revision touches upon all three of the above directions which adjustment may take the program may be of interest to *Bulletin* readers.

Considering the academic needs of women, men subject to early induction, Basic Course ROTC, and miscellaneous others, the Introductory Course, heretofore an indivisible two-semester 6-hour course, was changed to a one-semester course with four options for the second semester: advanced general, educational (for teacher training chiefly), social, and psychology of adjustment. In addition there is continued from last year the course in Military Psychology, repeated each semester, and *requiring no*

prerequisite. In the summer semester a 2-hour course is offered in Problems of Military Personnel. There is hence now made available for the student anticipating early induction (which *may* be postponed to end of academic term or year) four or five courses which may help him in OCS or specialist duties later. As military psychology is an innovation in American colleges, some observations on its nature are in order.

In view of the growing body of literature the organization of a course in military psychology now offers few of the difficulties met with a year ago. The experience of the Departments as at Illinois and Iowa can now be relied upon; also, material such as the earlier outline of Pennington and Case (**9**) which has now been enlarged upon as a text (**10**); and a new text recently published (**8**). The hand-book, "Psychology for the Fighting Man" (**2, 3, 14**) is useful as will also be the more extensive text to be prepared by Boring's Committee (**2, 3**). There are also the informative *Bulletin* articles published during the past months in the *Psychology and the War* section (**1, 4, 5, 7, 15**). The various approaches are different in nature, tending to supplement each other, and reflect the different training, background, interests and experience of the authors. The Illinois course featured learning with less treatment given other phases, such as personnel, leadership, and adjustment. Since the Iowa development was somewhat differently oriented it will be outlined in some detail.

In October, 1941, a committee was appointed by the late John A. McGeoch, composed of C. A. Strother, D. B. Stuit, and N. C. Meier (chairman) to organize a course in military psychology. With McGeoch as ex officio member, learning, clinical, personnel, and social psychology were hence represented, but as the work progressed responsibility devolved upon the chairman, the only member with military experience and contacts in the field. A portion of the course content—the section on morale—was offered during part of the 1941–42 academic year and the full course given both semesters in the following year.

As organized the course was designed to make the pertinent and usable principles and data of psychology (and closely related sciences) available to the interested student, particularly those in the Advanced ROTC. It featured motivation, morale, survival, adaptation of skills to military needs, and a comprehensive treatment of leadership both general and combat, with treatments also of learning, conflict behavior, psychological warfare, and adjustment. The catalog description is as follows:

103 *Military Psychology.* Data and principles of psychology applied to problems of modern warfare. Morale and other aspects of motivation; adaptation of civilian skills to military needs; factors in learning; perception, recognition, and illusion in field situations; psychological aspects of leadership, coordination, adaptability, and personality in military operations; problems of adjustment. No prerequisite. 2 semester hours.

The outline of the course, revised for 1943–44, is given below.

I. Why Men Fight; Group Conflict. An introduction, based on Stratton, Farago, and others, designed to afford a limited insight into frustration, nationalistic attitudes, resolution of frustration into conceptions of ideological conflict; and psychological preparation for war.

II. Psychological Aspects of Warfare. The human factor in fighting; intelligent military behavior; survival practices; strategy and psychological warfare; factors conditioning individual and group survival.

III. Psychological Preparation for Combat: Morale. Positive and negative incentives; re-directed innate drives; the enlistment of self-interest toward evoking maximum effort. Methods and practices for improving morale; expedients for sustaining morale under difficult conditions.

IV. Adaptation of Skills to Military Needs. Classification and placement of personnel; service requirements for special skills, capacities, abilities, and knowledge; requirements of infantry, paratroops, Airforce, motorized units, and communications; general field situations: observation, recognition and deception; camouflage.

V. Learning Military Skills. Factors in learning and forgetting; basic principles of military instruction; aids to effective teaching; development of good study habits, and self-improvement in learning ability.

VI. Leadership and Coordination. Theory of discipline; soldierly qualities; principles and traits of military leadership; considerations related to personality traits, courage, and the control over fear; leadership in higher command; problems of coordination; generalized aspects of leadership.

VII. Leadership in Combat Situations. Preparation for combat; safeguards against fear and morale deterioration; behavior on hazardous missions; control of rumor; prevention of panic impulses; adaptability and military tactical intelligence.

VIII. Adjustment to Combat Conditions and Stress. Delayed adjustment in training; individual differences in attitudes toward combat; preparatory psychological conditioning; fatigue and exhaustion; exhaustion neuroses; 'shell shock'—its nature, avoidance and treatment; other forms of incapacitation.

The course as now offered in its second year will probably include mostly 17- and 18-year old students taking the Basic ROTC course, with some unassigned Advanced ROTC (many of these have been returned to colleges pending availability of training facilities), Liberal Arts students, and a few women. It thus serves students anticipating induction, without competing with

the Introductory Course, and provides for those who do not desire to make psychology their major.

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BOOK REVIEWS

HALLOWELL, A. IRVING. The role of conjuring in Saulteaux society. Philadelphia Anthropological Society Publ., Vol. 2. Philadelphia: University of Pennsylvania Press, 1942. Pp. vii + 96.

The aim of this volume is twofold. First, the author proposes to report the function of one form (non-inspired, according to E. M. Loeb) of shamanism within a group of the Ojibwa Indians. Second, he aims to compare these conjuring practices with those employed by other tribal groups. The author, to accomplish these ends, presents the results of exhaustive personal observations (1930–1940) made in a group of 900 Indians in the Lake Winnipeg region. These descriptions of tribal séances are accompanied by plates of the heretofore unphotographed conjuring lodge ("shaking tent"). Throughout the narrative the author endeavors to set forth the "magico-religious beliefs" of these people and, hence, to delineate the nature of Saulteaux cosmology in relation to their culture. In so doing he commendably avoids judging Saulteaux societal practices in terms of our own norms.

Of particular value for the psychologist, interested in social and anthropological problems, are the following: (1) the nature and content of dream experiences that serve as signs of supernatural appointment to shamanism; (2) the nature of religious beliefs and the animistic, anthropomorphic, transmigrative, and reincarnative presuppositions that underlie each; (3) the present decrease in the number of conjurers and the reduction in the number of séances held by respective shamans as related to changes in Saulteaux society; (4) the mechanics of the conjuring pattern itself. The latter is of particular interest to those students of objective and subjective metapsychics. In addition, the social psychologist will be interested to note that conjuring is held to serve a fourfold role in Saulteaux society. First, it humanizes and reinforces belief in Indian deities. Second, it provides means for applying sanctions to nonconformists. Third, conjuring lends an element of security to those in distress. Finally, it plays a dramatic role and serves as entertainment. For these reasons Hallowell holds that conjuring is an "index of the vitality of Saulteaux beliefs, attitudes and values."

For this reviewer the monograph is essentially descriptive and illustrative. The author has not associated psychological and sociological principles with his well-documented evidence. The question is raised concerning the nature of the conjurer's personality, for example, but the results of the administration of the Rorschach Test are not mentioned. No serious attempt is made to account for the so-called metapsychical phenomena evidenced at the séance. The author, however, does achieve his aims—those of describing, comparing, and delineating the role of conjuring in Saulteaux society *per se*. The volume is well annotated and the list of references is well arranged and inclusive.

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SIEBER, SYLVESTER A., & MUELLER, FRANZ H. The social life of primitive man. St. Louis, Mo. & London: B. Herder Book Co., 1941. Pp. xiii +566.

This book, intended as an introduction to the study of sociology and economics, rather than as a textbook of anthropology, presents the point of view of the *Kulturekriesschule* and is based, according to the authors, on *Völker und Kulturen* by Fathers Schmidt and Koppers.

The members of what was once the Vienna school of ethnology hold that the study of the social life of primitive peoples "who have preserved in isolation the cultural stages of the past through which man has progressed" gives a better understanding of pre-history than does the study of "graves, tombs, kitchen-middens and other excavated sites." The primitive tribes are considered as "living cultural wholes." "Here we find not only the same implements, utensils, and other material objects that prehistorical investigations have dug up from the past but we see them woven into functioning culture patterns." (2)

The culture circle is "a culture complex which embraces all the essential categories of human life, economic, social, political, aesthetic, ethical and religious." (10) While embracing a circumscribed geographical area, the culture circle is "also a biological concept since it is so intimately connected with the life of whole tribes, which are social and biological organisms." (11) The primitive culture circles were based on food-gathering. The authors have described the social institutions of primitives and the various primary and secondary cultures.

The main argument of the book seems to be as follows: According to St. Thomas, man is by nature a social being. "Therefore he enters into unions and associations with other individuals. The family itself is fundamentally an association. In the last analysis, every community and the whole human race is rooted biologically in the family." (48) Totemism and the matrilineal and patriarchal cultures, which went contrary to the principles of the natural (monogamous) family were destined to a short-lived existence. The struggle of lower (economic) classes to rise, wars, slavery, prostitution, and other forms of social evil, were due to the development away from the primitive form of family life. Of the great religions of history, Christianity alone did not tend to crystallize these "one-sided social phenomena." The coming of the Christian religion, which brought the return of "monogamy, indissoluble marriage, censure of infidelity of husband and wife, freedom of both parties to a marriage, restoration to a position of dignity for both woman and child," (483) changed the picture of culture in the Western world. Christ "set in motion principles that were to affect the social history of the world and gave to society the only foundation upon which it could securely rest and long endure." (483)

An appendix on symbiosis and cross-cousin marriage and a glossary of terms are helpful to those whose training in anthropology has been limited. The index would have been improved by more complete reference to the names of persons quoted.

ARTHUR JENNESS.

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NOTES AND NEWS

MAX WERTHEIMER, professor of the graduate faculty of the New School for Social Research (New York City), died of a heart attack, October 12, at the age of sixty-three years. Dr. Wertheimer was head of the department of psychology at the University of Frankfurt from 1929 to 1933, and left Germany in 1933 to become one of the original members of the University in Exile. Dr. Wertheimer, after many years of work, had recently completed a major work in English on "Productive Thinking."

MARION L. MATTSON, director of the nursery school and associate professor of home administration and child psychology at Purdue University, died as a result of a railway crossing accident at Greencastle, Ind., November 1.

The September (1943) issue of the British Journal of Psychology, General Section, announces the death of KURT HUBER, professor of psychology in the University of Munich since 1926, who was beheaded for having inspired an anti-Nazi conspiracy.

The following promotions have been announced by Emory University (Ga.): M. C. LANGHORNE, and H. W. MARTIN, associate professors of psychology to professorships, and W. G. WORKMAN, assistant professor of psychology to associate professorship.

ROBERT P. FISCHER, formerly at Clark University, is now on the psychology staff of the University of Illinois.

HAZEL SCOFIELD CLAPP, formerly psychologist at Grasslands Hospital (Yonkers, N. Y.) has been appointed to the department of psychology at Briarcliff Junior College (Briarcliff Manor, N. Y.).

FRED A. REPLOGLE, for the past five years, personnel director, Macalester College (St. Paul), has joined the staff of Stevenson, Jordan, and Harrison, Inc., Management Engineers, Chicago, as consulting and clinical psychologist in servicing a wide variety of war industries.

A new course in personnel psychology, covering problems of selection and placement, including current applications of psychology to personnel problems of industry, the armed services and government agencies, is offered jointly during the autumn term by the Graduate School of Arts and Science and the Graduate Division for Training in Public Service of New York University at the Washington Square Center. The instructors, all associated with the Personnel Research Section, Classification and Replacement Branch, Adjutant General's Office, War Department, are LIEUT. COL. MARION W. RICHARDSON, officer in charge, Personnel Research Section; CAPT. ROGER M. BELLOWS, officer in charge, Technical Classification Unit, and DR. EDWIN R. HENRY, chief, Technical Classification Unit.

NOTICE

Because of restrictions on the use of paper it has been necessary to hold over several short papers and book reviews originally scheduled for this issue, for later publication.

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